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From December 11, 1884, *to June* 18, 1885.

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ERRATA.

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Page 394, line 13 from bottom,—*for* "Stuart" *read* "Smart."

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Rev. R. Harley, F.R.S., "Professor Malet's Classes of Invariants identified with Sir James Cockle's Criticoids":—

Page 49, line 6,—*for* "1_1, 0, H_1," *read* "1, 0, H."

Page 50, art. 11, line 11,—*for* "x" *read* "x'," viz.,

$$Q_3 = P_3(x')^3.$$

Page 50, last line, and page 51, line 5,—*for* "Q_1" *read* "Q'_1."

Page 52, lines 9 and 10,—*insert*

$$\{n-1)\lambda + (n-2)\mu - 1\}\frac{P_1T'}{T^2}$$

Page 53, art. 13, line 2,—*for* "P_n" *read* "P'_n."

Page 55, line 2, and page 56, line 2,—*for* "$Q' + \ldots$" *read* "$Q'' + \ldots$"

Professor Stokes, F.R.S., "On a Remarkable Phenomenon of Crystalline Reflection":—

Page 178, line 29,—*for* "diffraction" *read* "refraction."

PROCEEDINGS

OF

THE ROYAL SOCIETY.

December 11, 1884.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Absorption-spectra of the Alkaloids." By W. N. HARTLEY, F.R.S., Professor of Chemistry, Royal College of Science, Dublin. Received November 19, 1884.

(Abstract.)

While studying the molecular constitution of various organic substances by means of their action on the ultra-violet rays in the manner described in the "Philosophical Transactions," vol. 170, p. 257, 1879, it was considered of importance to ascertain whether absolute physical measurements could not be substituted for the uncertain chemical reactions and variable physiological tests at present employed as a means of detecting the alkaloids in medico-legal examinations. About forty alkaloids and derivatives therefrom have been examined, authentic specimens having been procured from the chemists by whom they were prepared. Solutions were carefully made of the same strength in most cases, only diactinic solvents, most generally alcohol, being employed. The cells with quartz sides for holding the solutions were of various thicknesses, ranging from 1 mm. to 20 mm. The electrodes employed to give a well-defined spectrum consisted, one of an alloy of tin with 25 per cent. cadmium, the other of lead with cadmium in the same proportion. Spectra are thus obtained with lines of the same intensity, numerous and evenly distributed throughout a spectrum extending

from wave-length 4414·5 to 2145·8. The prominent lines of cadmium are distinguished by their extension across the spectrum from pole to pole, while those of lead are on one side only and those of tin on the other. As a weaker continuous spectrum fills the intervals between the lines, there is no difficulty in obtaining accurate measurements. To secure well-defined spectra the photographs were taken with the solutions placed in front of the slit, upon which the rays from the sparks were concentrated by a quartz lens of 2 inches diameter and 3 inches focal length. The spectra were measured by means of an ivory scale applied to the surface of the photographs; this had bevilled edges and was divided thereon into hundredths of an inch. The linear measurements are termed scale numbers and are arbitrary, but they were reduced to wave-lengths by the use of an interpolation curve. The oscillation frequencies were also read off on a second curve whenever it was considered desirable to record them. The wave-lengths were taken from those published in the "Philosophical Transactions," vol. 175, p. 63, 1884, but for use in recording these measurements the fractions of a tenth-meter were disregarded. The total number of lines employed, including two or three air-lines, was seventy. For the convenience of those who may be engaged in similar work, the wave-lengths of the lines and their reciprocals are given on page 3. The wave-lengths of a magnesium line and a calcium triplet are also inserted, as it is sometimes convenient to refer to them.

The absorption curves which have been drawn differ from those figured in my previous communications, owing to the use of wave-length numbers. The curves have been made continuous, and so the necessity for shading has been avoided; but very careful descriptions of the spectra are furnished in addition, so that no detail has been omitted. Nearly all the samples of alkaloids examined were obtained from Messrs. T. and H. Smith and Co., of Edinburgh, Mr. David Howard, of the firm of Howard and Sons, of Stratford, and Dr. C. R. A. Wright, F.R.S. The bodies may be divided into two groups, those which exhibit spectra with absorption-bands and those with continuous spectra.

Alkaloids and Derivatives exhibiting Absorption-bands.

Aconitine.	Oxynarcotine.
Pseudaconitine.	Apomorphine Hydrochloride.
Japaconitine.	Cotarnine Hydrobromide.
Morphine.	Tetracetyl Morphine.
Narcotine.	Diacetyl Codeïne.
Codeïne.	Quinine.
Thebaïne.	Quinine Sulphate.
Papaverine.	Cinchonine Sulphate.

Quinidine Sulphate.
Cinchonidine Sulphate.
Veratrine.
Piperine.
Brucine.
Strychnine.

Alkaloids transmitting continuous Spectra.

Narceïne.
Aconitine (foreign).
Cevadine.
Atropine.
Solanine.
Hyoscyamine.
Digitaline.
Picrotoxine.
Nicotine.
Caffeïne.

Scale numbers.	Wave-lengths.	Reciprocals.	Scale numbers.	Wave-lengths.	Reciprocals.
17	4480 Mg	2232	190	2812 Sn	3556
17·7	4454 Ca	2245	192·5	2801 Pb	3571
18·9	4434 Ca	2255	197	2778 Sn	3599
20·0	4424 Ca	2260	203·5	2747 Cd	3640
20·5	4414 Cd	2265	213	2705 Sn	3696
22·5	4386 Pb	2280	223	2662 Pb	3756
30·5	4245 Pb	2355	235	2613 Pb	3827
42·7	4061 Pb	2462	240	2593 Sn	3856
62·0	3800 Sn	2503	244	2576 Pb	3882
67·3	3739 Pb	2674	245	2572 Cd	3888
72	3683 Pb	2715	245·5	2570 Sn	3891
76·3	3639 Pb	2748	247	2561 Pb	3904
79	3610 Cd	2770	251·5	2545 Sn	3929
82·7	3572 Pb	2799	266·5	2495 Sn	4008
93·7	3465 Cd	2886	270	2483 Sn	4027
97	3437 Air	2909	272	2475 Pb	4040
101	3403 Cd	2938	281·3	2445 Pb	4090
106·5	3352 Sn	2983	282	2443 Pb	4095
109·5	3330 Sn	3003	286·5	2429 Sn	4116
115	3283 Sn	3045	289	2422 Sn	4128
118	3262 Sn	3065	295	2402 Pb	4163
119·5	3260 Cd	3067	298	2393 Pb	4178
129·5	3174 Sn	3150	306	2368 Sn	4223
135	3137 Pb	3187	311	2355 Sn	4246
151	3033 Sn	3297	318	2335 Sn	4282
155	3008 Sn	3324	320	2329 Cd	4293
159·5	2980 Cd	3355	322·7	2321 Cd	4308
165	2949 Pb	3391	325·7	2313 Cd	4323
171	2912 Sn	3434	335	2288 {Cd, Sn}	4370
177	2880 Cd	3472			
178·5	2872 Pb	3481	344	2265 Cd	4415
180	2862 Sn	3494	351	2247 Sn	4450
182·5	2849 Sn	3510	353·5	2241 Cd	4462
185	2837 Sn	3524	368·5	2205 Pb	4535
186	2832 Pb	3531	372·5	2195 Cd	4555
188	2822 Pb	3543	395	2145 Cd	4662

The conclusions to be drawn from this investigation are the following:—

1. The absorption-spectra offer a ready and valuable means of ascertaining the purity of preparations of the alkaloids, and particularly of establishing their identity.

The quantity of some of the alkaloids present in a solution may be estimated by means of the absorption curves.

The different character of the various specimens known as aconitines may be recognised; thus the comparatively harmless base may be distinguished from those of great physiological activity by its transmission of a continuous spectrum, while the three specimens of physiologically active aconitines are distinguished from one another by their characteristic absorption curves.

That the three active aconitine bases are substances each with a different chemical constitution, is a conclusion confirmed by optical examination.

The purity of quinine and absence of any admixture of cinchonine can be readily determined by reason of the latter substance being much less diactinic than the former; but for the same reason quinine cannot be estimated in presence of cinchonine. Drugs of such potency as aconitine, morphine, quinine, strychnine, &c., which ought to be prescribed only when of absolute purity, should have their exact nature and degree of purity guaranteed by an examination of their absorption-spectra.

2. In comparing the spectra of substances of similar constitution, it is observed that such as are derived from bases by the substitution of an alkyl radical for hydrogen and acid radicals for hydroxyl, the curve is not altered in character, but may vary in length when equal weights of substances are examined. This is explained by the absorption-band being dependent upon the compactness of structure of the carbon and nitrogen nucleus of the molecule, and because equal weights are not molecular weights. Examples are afforded by morphine and codeïne (methyl-morphine), diacetyl-codeïne, and tetracetyl-morphine.

3. Bases which contain oxidised radicals, as hydroxyl, carboxyl, or methoxyl, diminish in diactinic quality in proportion to the amount of oxygen they contain. Examples are papaverine, narceïne, narcotine, and oxynarcotine.

The apo-derivatives are less diactinic than the parent bases in a degree which indicates that the molecular weights have been nearly doubled. Examples are apo-morphia and pseudaconitine.

4. Bodies with the pyridine and quinoline nucleus exhibit absorption-bands extending between wave-lengths 350 and 280, those with a benzene nucleus generally from 290 to 260, or rays even more refrangible; while the aconitines and opium bases, likewise strychnine, give evidence of a benzene nucleus, the cinchona bases, with piperine and brucine, appear to contain a nucleus of quinoline or pyridine.

II. "On the Function of the Thyroid Gland." By Professor VICTOR HORSLEY, B.S., M.B., F.R.C.S. Communicated by Professor M. FOSTER, Sec. R.S. Received December 5, 1884.

(Preliminary Communication.)

Up till the year 1883 the function of the thyroid gland was unknown, and considered to be of slight importance, at least to the adult animal.

The theories concerning its function were—

(1.) The one propounded by Mr. Simon, "Phil. Trans.," 1844, &c., viz., that the thyroid body acted as a regulator of the circulation in the brain, and possibly manufactured some substance which was of primary importance for the nutrition of the central nervous system.

(2.) That it was a true gland, and secreted a mucinous albuminoid into the cavities of its acini, the secretion being reabsorbed by the lymphatics.

(3.) The thyroid gland has also from time to time been compared to the spleen as an hæmapoietic organ.

Although the intimate relationship of goitre to cretinism has been well known for many centuries, the fact that excision of the partially goitrous thyroid in comparatively healthy people was followed by severe symptoms of cretinism was first announced last year by Professor Kocher, of Berne, who collected 160 cases in which the operation had been partially or wholly performed. In the latter class of his own cases he found the patients, without exception, had become cretinous.

The symptoms described by Kocher were recognised by Dr. Semon* as similar to those of Myxœdema, and he advanced the theory that the conditions were allied. My experiments have proved the truth of this view, since I have produced the condition of myxœdema by simply excising (with strict antiseptic precautions and operating so as to avoid all nerve trunks) the thyroid gland in the monkey.

Schiff in Geneva, Wagner in Vienna, and Sanquineco and Canalis in Turin, have made similar experiments on dogs, but they do not appear to have found the myxœdematous condition; simply, it appears, because, for one reason or another, their animals did not survive the operation long enough to develop the pathological changes.

However, they show that in the dog careful ablation of the thyroid gland is followed by severe nervous symptoms, which commence a few hours, or days, after the operation, by fibrillar contractions and

* "Brit. Med. Jour.," Nov. 30, 1883.

tremors in all the muscles of the body. These tremors soon become clonic spasms, and ultimately paroxysms of true tetanus, as a result of which the animal dies a few days after the operation.

It is clear then that in dogs the thyroid gland has an intimate connexion with the central nervous system.

In the monkey, excision of the thyroid, just as in the dog, may be followed immediately by fibrillar contractions of the muscles of the extremities, but, as a rule, the animal remains perfectly well for five days.

The tremor at its first appearance is uniform, and has a wave rate of 8–10 per second.

In forty-eight hours, as a rule, it becomes paroxysmal, *i.e.*, exhibits recurring exaggerations, the paroxysms being of variable duration and interval, but giving a new character to the tracing, owing to the powerful movements of the limbs, &c.

The uniform tremor persisting between the paroxysms now has, as a rule, a wave rate of 16 to 20 per second.

These symptoms persist, as a rule, about twenty days; they then decline, the paroxysms disappearing slowly, but ceasing before the constant tremor.

During the whole of this time there is rigidity and paresis* of the muscles affected.

Moreover, the symptoms of the disease termed Myxœdema by Dr. Ord, and ordinary cretinism are gradually developed.

For—

The animal becomes gradually more and more imbecile and apathetic, sitting, as it does, huddled up and taking no notice of anything, in strong contrast to its customary vivacious state.

It exhibits swellings of the skin of the face, abdomen, &c., due to infiltration of the tissues by mucin. (This change, visible to the naked eye, has been chemically established by my friend Dr. Halliburton, B.Sc., whose results are published in accompanying table.)

The salivary glands become enormously hypertrophied, and the parotid gland, which normally secretes a watery, serous fluid, now takes up a muciparous function,† and produces quantities of mucin.

This increase of function is interesting, as probably offering a clue for further investigation into the physiology of secretion.

The blood is profoundly changed: there is a decrease of red corpuscles, and a primary increase of the leucocytes, followed by a decrease, oligæmia thus resulting. Moreover, it contains mucin in proportion to the duration of life after the operation, and the serum albumin is diminished.

* Sometimes almost total paralysis of the extensor muscles.

† Microscopic investigation shows the cells of the parotid to be swollen by mucinogen (?) and rapidly disintegrating.—14th December, 1884.

The temperature, slightly raised by the operation, becomes variable, and then after about twenty-five days, gradually sinks far below the normal, and the animal dies comatose.

It is proved by examination of the parts that in the operation the thyroid gland only was removed, the surrounding structures being uninjured; consequently the thyroid gland may now be looked upon as being of extreme importance in the animal economy, and especially in relation to three points—

(1.) The nutrition of the nervous system.
(2.) The existence of mucin in the body.
(3.) The composition of the blood.

The practical surgical question as to whether the cretinous symptoms following thyroidectomy are due to—

(1.) Chronic asphyxia, as believed by Kocher;
(2.) Injury of the sympathetic and other nerve trunks ;
(3.) Arrest of function of the thyroid gland;

is almost settled in favour of the third view, and with it also the pathology of Myxœdema.

3rd December, 1884.

Analysis of Tissues (Monkey) Healthy and after Thyroidectomy.
By Dr. W. D. Halliburton, B.Sc.

Amount of Mucin per 1000 parts of the tissue.

Normal.	Skin and subcutaneous tissue.	Tendon.	Muscle.	Parotid gland.	Submaxillary gland.	Blood.
No. 1*a*. Healthy monkey (killed).	·89	·39	0	..	..	0
No. 9. " " "	·9	·5	0	0	·01	0
Abnormal, after Thyroidectomy.						
No. 1 lived 55 days	3·12	2·55	0	·72	6·0	·35
" 3 " 32 "	..	..	..	..	..	trace
" 5 " 49 "	2·3	2·4	trace	1·7	3·3	·08
" 10 " 7 "	..	..	0	..	..	merest trace

The tissues were minced finely, and kept under alcohol for one week; the mucin then was dissolved out by baryta-water and reprecipitated by acetic acid. The precipitate from known weights of tissue was thoroughly washed, and then dried at 100° C., to give the quantitative result. The chemical research was carried out in the Physiological Laboratory of University College, London.

III. "On the Development of the Arteries of the Abdomen and their Relation to the Peritoneum." By C. B. LOCKWOOD. Communicated by W. S. SAVORY, F.R.S. Received November 18, 1884.

(Abstract.)

The paper begins by explaining that it records an endeavour to elucidate the course and relations of the abdominal blood-vessels upon developmental grounds. The earliest stages of the development of the midgut and its mesentery are first described; and it is shown that vessels extend at very frequent intervals from the dorsal aorta to the intestine, reaching the latter by way of the mesentery. The development of the stomach is mentioned, and the gastric artery is identified as one of the original arteries of the mesentery. The effects which the various alterations in the position of the stomach have upon its blood supply are discussed, and are considered to account for peculiarities in the course of the gastric artery. The splenic artery is reviewed next, and it is explained how both it and the spleen are developed between the layers of the mesogastrium; evidence is adduced to show that the vessel, even in the adult, reaches its destination by passing through the mesogastrium, and it is maintained that this fact determines the relations of the artery to the greater and lesser cavities of the peritoneum. Next the very long arterial loops of the great omentum are discussed; as the omentum is nothing but the elongated mesogastrium it is argued that they represent the original vessels of that fold. The hepatic artery comes next, and the protrusion of the liver from the midgut is described, and afterwards the development of the lesser omentum. The hepatic artery is shown to be one of the original arteries of the mesentery of that part of the gut from which the liver grew. This fact is shown to explain the relations of the vessel to the peritoneum and foramen of Winslow, and also it is made use of to explain certain abnormalties of the artery. It is pointed out that as the original function of the hepatic artery in the embryo is to supply the bowel so it continues to afford branches to it in the adult. The arteries of the small intestine and pancreas are reviewed. With regard to the latter it is explained how the organ is developed from the duodenum, and extends along the mesoduodenum into the mesogastrium where it impinges upon the spleen and its artery; this, it is argued, accounts for the sources of its blood supply. The manner in which the pancreas becomes situated behind the peritoneum instead of between the layers is afterwards attributed to the unfolding of the peritoneum between the mesogastrium and the transverse mesocolon (*i.e.*, the original

mesentery of the transverse colon before it becomes related to the omentum). The colic arteries are briefly dismissed with the exception of the middle; this vessel is discussed at greater length owing to the doubts which exist concerning the development and anatomy of the transverse mesocolon. An attempt is made to refute the adhesion theory advocated by Haller, and to explain how it is quite possible for the mesocolon to consist of two layers instead of four.

In conclusion the following principles are deduced:—

First. That the arteries of the abdomen, including the splenic and hepatic, were originally derived from the dorsal aorta for the supply of the midgut.

Second. That they reach their destinations by passing through the mesentery.

Third. That they participate in all the changes the mesentery undergoes.

Fourth. That if an organ is developed in the mesentery or from the gut, it will obtain part at least of its blood supply from the vessels of the mesentery or gut, and that these will conform to the preceding rules.

It is finally remarked that although these observations have only been applied to the human subject, yet they appear so simple and so likely to be true that it is probable that they have a much wider applicability.

IV. "On the Occurrence of a Hydroid Phase of *Limnocodium Sowerbii*, Allman and Lankester. By ALFRED GIBBS BOURNE, Assistant Professor of Zoology in University College, London. Communicated by E. RAY LANKESTER, M.A., F.R.S. Received December 8, 1884.

It is now four and a-half years since the Medusæ of *Limnocodium* were discovered by Mr. Sowerby, in the Victoria tank in the gardens of the Royal Botanic Society, at Regent's Park.

Since that time Professor Lankester has continued to make observations and experiments, in which I have assisted, with the view of elucidating their life-history and the conditions of their somewhat anomalous occurrence. We discovered, however, nothing which threw any light upon the subject. On November 27th we had an opportunity of seeing the tank at the Botanical Gardens, immediately after the withdrawal of the water, and I then collected and examined at Professor Lankester's request a large quantity of the sediment and portions of various roots and other submerged objects.

The Hydroid Form.

I discovered upon some of the root filaments of the plants of *Pontederia* which were growing in the tank in great profusion, an organism, which in its method of growth somewhat resembles an encrusting sponge. It occurs very abundantly upon some of the roots, and is not to be found upon others. I am unfortunately unable to say whether the roots upon which it occurs were near together or scattered through the tank. I have not seen it upon any young roots, while on the other hand it is most abundant upon the oldest and blackest roots. Where most abundant there are four or five

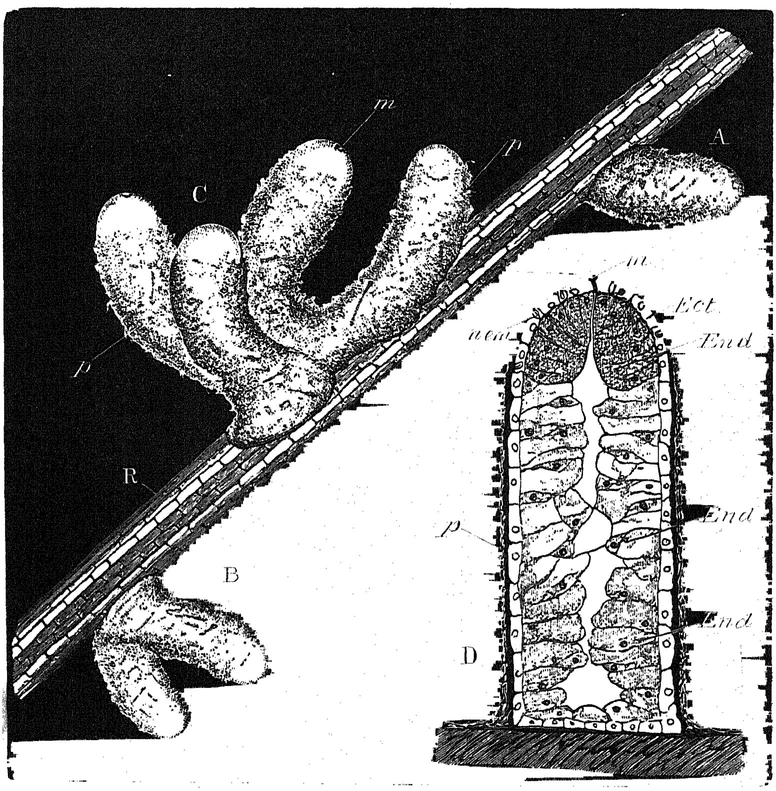

Fig. 1. Hydroid forms of *Limnocodium Sowerbii.*

R. Root filament of *Pontederia;* A, B, C. Colonies of various sizes; D. Portion of a Hydroid in optical section, slightly diagrammatic; *Ect.* Ectoderm; *End.* Endoderm; *m.* "mouth;" *nem.* Nematocysts; *p.* Perisarcal tube of adventitious particles.

pieces of different size and shape upon any one filament. The smallest pieces are mere knobs, and about $\frac{1}{50}$ of an inch long; the largest are prolonged into three or four lobes, about $\frac{1}{8}$ of an inch long.

No tentacles are present. They may develop subsequently. The basal portion tends slightly to spread over its area of attachment, this portion half encircling a root-filament.

There is no trace of a true perisarc, but the surface is covered with particles of mud and other *débris*, which become glued together by some secretion of the animal, and form a sort of tubular casing. In some specimens which I have mounted I have observed this casing slipping off, owing to shrinkage of the animal.

The tip of a lobe usually projects beyond this tube, but sometimes in specimens placed in cold water it withdraws itself sluggishly into the tube.

The organism does not exhibit any active movement, but upon irritation there is a slight contraction, such as described above, and accompanying this there may be an expulsion of fluid and minute particles from the extremity of each lobe, demonstrating the existence of an aperture to each, which presumably serves as a mouth.

The animal throws off nematocysts when irritated.

Preparations of the stained animal mounted whole and serial sections have revealed further points in its structure.

The ectoderm cells are fairly uniform throughout, and present numerous nematocysts.

The endoderm cells are much modified in the "mouth" region, *i.e.*, at the apex of each lobe, being set very closely together, and staining very deeply with carmine.

In preserved specimens there is only a very minute lumen, leading from the terminal aperture.

Lower down the endoderm cells have a clear swollen appearance, and almost fill up the enteric cavity. In the basal region the endoderm cells are normal, and the cavity is well developed, and may contain particles. The peculiar arrangement of endoderm cells near the apex may have no special significance, but it somewhat suggests in appearance the rudiment of a sub-umbrella found in such forms as *Hydroctinia*, *Podocoryne*, and *Hippopodius*, figured by Ed. Van Beneden, and more recently by Weissmann.*

Structures resembling Buds produced by the Medusiform Persons.

During the summer of 1883 I discovered both in water taken direct from the tank at the Botanic Gardens and in jars in which the Medusæ had been kept alive for some time, minute vesicles consisting of ectodermic and endodermic layers, surrounding a closed central

* Die Entstehung der Sexualzellen bei den Hydromedusen.

cavity in which particles were apparently undergoing digestion. Between the ectoderm and endoderm more or less gelatinous tissue was always developed, separating the two layers. I was prevented from carrying my observations very far, owing to the sudden disappearance of all the Medusæ from the tank. I therefore propose to reserve further details for a future occasion. It may, however, be useful as a guide for further observations if I state that I ascertained these vesicles to be formed by a nipping off of the genital sac after the liberation of the ripe spermatozoa. I kept these vesicles in isolated vessels and examined them from day to day for four or five days, during which they underwent certain changes, but these changes were somewhat irregular, an irregularity owing possibly to the unfavourable conditions, possibly to the lateness of the season, all Medusæ being about to disappear.

The mere fact that all the Medusæ I could keep long enough under observation threw off all their genital sacs in a similar way, and that these vesicles lived for so long a time without disintegrating, points to this being some regular process of reproduction.

I repeated part of these observations during last summer, but was unable to continue them, owing to the short period during which the Medusæ flourished this year.

History of Limnocodium to the Present Date.

The following table gives the history of *Limnocodium* up to the present date. For many of these dates I am indebted to Mr. Sowerby.

March 1879.	*Pontederia* plant introduced into the tank (R.B.S.).
March 15, 1880.	Tank (R.B.S.) filled after remaining empty during the winter.
June 10, 1880.	Adult and very young Medusæ observed in immense numbers.
July 31, 1880.	Medusæ had all disappeared.
Dec., 1880.	Tank (R.B.S.) emptied, damp mud and roots only being left.
March 17, 1881.	Tank (R.B.S.) filled.
June 12, 1881.	A few adult Medusæ observed; swarms of young.
June 16, 1881.	About eight adult Medusæ, numerous young forms, and some mud, transferred in a jug of water to the Victoria Tank at Kew.
June 25, 1881.	All Medusæ vanished at Regent's Park.
Aug. 18, 1881.	Adult Medusæ swarmed at Kew.
Sept. 30, 1881.	Kew tank contained adult forms only.
Oct. 15, 1881.	Kew tank emptied, and a few Medusæ only seen.

March 10, 1882.	Tank filled at Regent's Park.
	No Medusæ observed anywhere during this year (1882).
March 8, 1883.	Tank filled at Regent's Park.
April 28, 1883.	Medusæ appeared at Regent's Park, adult and young.
July 13, 1883.	Nipping off of spheres from the genital sacs first noticed.
July 14, 1883.	Ten adult Medusæ in clean water transferred to Kew. These were never seen again.
July 25, 1883.	All Medusæ disappeared. The tank at Regent's Park was not emptied during this winter.
April 27, 1884.	Two specimens of adult Medusæ observed at Regent's Park.
May 28, 1884.	Adult and young Medusæ next observed, and in great numbers.
June 20, 1884.	Very few Medusæ observed.
June 30, 1884.	Medusæ almost had disappeared.
Nov. 28, 1884.	Hydriform organism seen on *Pontederia* roots when tank at Regent's Park was emptied.
Nov. 30, 1884.	*Pontederia* root bearing hydroid placed in Kew tank.
Dec. 1, 1884.	Tank refilled at Regent's Park.

General Remarks.

That this new and in many ways most remarkable hydroid, appearing in fresh water in great quantities, in the tank where *Limnocodium* appeared, and upon the roots of a plant which was introduced just before *Limnocodium* appeared, is a phase in the life-cycle of *Limnocodium* seems so highly probable, that we may take it for granted at present. Incontestable proof will, I hope, be furnished next spring by tracing its further development.

As it seems extremely probable that during next summer we may be able to explain fully the life-cycle of this most interesting organism, it is not necessary to devote much time now to speculating upon what that cycle will probably be.

The connexion of the organism with *Pontederia*, first suggested by Professor Lankester, seems to be borne out by the fact that only upon the *Pontederia* roots can we find the hydroid form. That the *Limnocodium* did not establish itself at Kew may be owing to the fact that there was no *Pontederia* in that tank.

In dealing with the hydroid form here described, we must remember that *Limnocodium* is undoubtedly one of the Trachomedusæ. No trachyline form has before this been shown to be connected with any hydroid condition, but we know less about the Trachomedusæ than

about the Medusæ derived from Gymnoblastic or Calyptoblastic hydroids. Direct development has been shown to occur in two or three genera of the Trachylinæ (*Geryonia*, *Ægineta*, &c.), whereas regarding the life-cycle of the other forms we know nothing.

The hydroid here described is now in an immature condition. It may be a creeping stolon, which when further developed will resemble other hydroids; it may turn out not to be comparable to other hydroids, and to be a fixed sporosac of some kind; but if it grows into an adult form resembling the form at present known, it may serve as the type of a new group of hydroids related to the Trachyline Medusoids.

December 18, 1884.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

It was announced that the question of the re-admission into the Society of Mr. James Bateman would be put to the vote at the next meeting.

The Chairman gave notice that the President had appointed as Vice-Presidents—

The Treasurer.
Mr. Warren de la Rue.
Sir Frederick Evans.
Professor W. H. Flower.
Sir Joseph Hooker.

Pursuant to notice, Mr. Alfred Cornu and Professor James Dwight Dana were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "On the Evidence of Fossil Plants regarding the Age of the Tertiary Basalts of the North-East Atlantic." By J. STARKIE GARDNER. Communicated by Sir J. D. HOOKER, K.C.S.I., F.R.S. Received November 27, 1884.

In the following communication I desire to make known to the Society some of the results which I obtained by means of grant given to me by the Government Grant Committee. Brief and incomplete

as these notes on the fossil plants must necessarily be at the present stage, they will, I trust, serve to disclose how enormously increased our knowledge must become when the study of these plants shall have been completed. An interpretation of them, very different to that current prior to the investigations undertaken by means of the grant, is now possible, affecting questions far beyond the immediate issues. That this preliminary notice may be confined to the briefest treatment that the subject admits, I have refrained from encumbering it with purely geological matter, and as far as possible I have avoided theoretical considerations. The former I have embodied in two papers to be laid before the Geological Society, and the latter I have already discussed in a lecture to the Belfast Field Naturalists' Club, I am happy to think with the result that many of the subjects pointed out as requiring further investigation have already received attention that bids fair to set some of the most unsettled questions at rest.

Ballypalady.

The collection I have made consists of several hundred specimens, procured mainly from the piles of ore waiting shipment on the quays at Belfast, as well as *in situ* in the quarries. Mr. Stuart, F.L.S., has since collected for the Belfast Museum, and has allowed me to make the fullest use of the specimens, but I have not yet thoroughly examined the collections in Dublin previously made by Mr. Baily. The present collection could be largely and easily supplemented. The plants occur in a ferruginous and indurated sandy clay, and the matrix is not very favourable for the preservation of the fine venation of leaves.

Ferns.—These are very rare. A macerated pinna indicates a *Pteris*, with undulating mid-rib and crenate margin, belonging probably to an Arctic fossil species. Other fragments agree with *Benitzia minima*, Saporta and Marion, from the Heersien stage of Gelinden.

Conifers.—These are relatively very numerous, and six plates and a half have already been published of them by the Palæontographical Society. The *Cupressineæ* are represented by a very abundant form, indistinguishable from the existing *Cupressus torulosa* of the Himalayas, and probably identical with the *Chamæcyparis belgica* of Saporta and Marion. The somewhat starved *Cryptomeria*, formerly *Sequoia Du Noyeri*, is the only member of the *Taxodieæ*, and seems identical with the *Sequoia subulata* of the Arctic so-called Upper Cretaceous. Some twigs look like *Taxus*. There are two *Pines*, both characteristic of the warmest regions in which Pines now exist; there are some leathery cones referable to *Tsuga*, and a seed very possibly of *Abies*. All except the latter and Taxus are very abundant. Mr. Baily includes *Taxodium* in his list, but I do not know upon what grounds, and the

figure of *Torellia rigida*, by Baily, is certainly no more than a fragment of a monocotyledonous leaf.

Monocotyledons.—These are of the usual character, proving that several reedy plants are present. One of these is evidently the plant known as *Typha latissima*, and in some cases the reticulated venation is exquisitely preserved.

Dicotyledons.—The leaves of about five-and-twenty species of trees or shrubs have been obtained, a fourth of which may perhaps eventually prove determinable. There is fruit or foliage of an *Aluns*, apparently identical with the Arctic fossil *A. Kefersteini.* The most abundant leaf does not seem distinguishable from *Celastrophyllum Benedeni*, Saporta and Marion, of the Gelinden Heersien, and one of the widespread type of leaves known as *Cinnamomum* is represented. The same water-lily, *Nelumbium Buchii*, Ett., that is met with at Monte Promina and in the Aquitanian of Switzerland, is also present here. Of the remainder, the pieces of *MacClintockia* and of the oak-like leaf of Glenarm *Quercus grœnlandica* (?) deserve notice, and are relatively of small size. Most of the leaves in this deposit are in fact small, and none are palmate or lobed.

The most remarkable fossil is an echinated globose body, which appears to be a fruit varying in size from a pea to a damson. It is sprinkled in great profusion over some of the slabs, in the shape of very deep cavities, which I for some time thought to be air-bubbles. The larger ones seem to have been lobed. All other fruits, even the fir-cones, are highly compressed, but this one is not in the least so, and must have thoroughly resisted pressure until the beds were quite consolidated. No trace of lignite, nor of other matters, remains in the casts, though the impress seems that of an organic body, yet it seems hardly possible to imagine any kind of plant to which it could have belonged, so variable in size and of such strange consistence must it have been. In spite of its organic look, there must remain doubt as to whether these cavities may not be due to the removal of some inorganic crystalline body.

Glenarm.

The mine whence these fossils are obtained had been under water for several years at the time of my first visit. The collection made by Mr. Baily for the British Association was from the spoil-bank, and also a few possessed by Mr. Gray. Mr. Swanston accompanied me on a second visit, and fragments obtained by us from the spoil-bank decided me to get the mine freed from water if practicable. Mr. Walter Jamieson, of the Eglinton Works at Glenarm, kindly assisted, and a fortnight's work enabled me to dig specimens from the bed *in situ*.

Ferns.—A week's work fortunately put me in possession of several

well-preserved pinnæ of a fern, seemingly identical with *Pteris Grœnlandica*, Heer, and very characteristic of the Arctic Tertiaries.

Conifers.—The only conifer and the prevailing plant is a *Cryptomeria*. The branchlets are densely tufted and sub-parallel, forking at very acute angles, and frequently bearing cones. There is apparently no character to distinguish it from the existing species. It was named *Sequoia Du Noyeri* by Mr. Baily, and doubtless some of the foliage from Arctic Tertiaries referred to *Sequoia* may also belong to it. The range of the existing genus is so restricted and definite, that its discovery in a fossil state in Ireland is of special interest.

Monocotyledons.—These are abundant, but afford no recognisable characters. Two or three diverging rush leaves side by side on the matrix sometimes almost appear to be a fragment of a palm leaf, and I cannot resist the suspicion that Heer may have been misled by a similar appearance into describing palms as occurring in the Tertiary flora of Greenland. They would, however, not easily be mistaken by one accustomed to collect fossil palms in the field.

Dicotyledons.—The prevailing form is a large pinnatified deciduous leaf, with a close and even texture, such as that of the *Plane*, *Chestnut*, or *Tulip-tree*. It is very variable in form, though scarcely more so than many of the *Oaks*, which it most resembles, or even the *Spanish chestnut*. Pealings of a thin bark are not uncommon, and this with the absence of anything like acorns leads to some hesitation in adopting Heer's determination of leaves, which are almost its counterpart, as *Quercus Furuhjelmus*, from Alaska, and *Quercus Grœnlandica*, from Atanekerdluk. Fragments of this one leaf have been named from Ireland alone as *Platanus Guillelmœ*, *Acer*, *Quercus*, *Sassafras*, and probably *Alnus Kefersteini*. Though there are no fruits to furnish a clue to its genus, it was probably a tree of large dimensions.

A rather smaller and more uncommon leaf has simple margins and more leathery texture, but cannot probably be determined generically. The most interesting leaf is that known as *MacClintockia*, or *Daphnogene Kanii*, a lanceolate leaf, with three or more mid-ribs, connected by an irregular network of veins. The examples are very fine indeed in this locality, a perfect leaf measuring nearly a foot in length, while some imperfect ones seem as if they may have exceeded this to double. The same species occurs in the Arctic floras and in the Heersien stage of the Lower Eocene of Gelinden. Its true affinities seem still unknown, but it does not seem to have been an evergreen, and somehow suggests rather a shrubby than an arborescent growth. A small ovate dicotyledonous leaf, devoid of character, has been figured as *Fraxinus*, by Mr. Baily, and other genera mentioned as occurring in these beds are *Corylus* and *Rhamnus*. Nothing came to light while I was working at the bed to suggest either of these genera except a fragment in the spoil-bank, which might possibly represent the former.

The Lough Neagh Beds.

The flora of these beds presents characters that are difficult to reconcile. The beds have been pierced to a depth of nearly 300 feet without base, and their accumulation must therefore represent a vast interval of time, during which many changes in the character of the flora of the surrounding country may, or I might venture to say must, have taken place. The flora is evidently one of very great variety, and almost entirely dicotyledonous. Though plant remains appear to be abundant throughout the entire thickness, we are only acquainted with them through their impressions in ironstone nodules, picked up on the shore of Sandy Bay, and these it seems likely are for the most part from the upper beds of the formation. I have determined but very few of them.

Ferns.—Two species have been described by Mr. Baily: one, *Lastræa stiriaca*, ranges in time from the Middle Eocene to the Miocene, and in distance from Greenland to Switzerland and Devonshire, where it abounds at Bovey Tracey. The second, *Goniopteris Bunburii*, Heer, is rare at Bovey, but commoner at Bournemouth, and does not, I think, occur in Greenland.

Conifers.—I have not seen any conifer myself, but Mr. Baily's list mentions *Sequoia Couttsiæ*, which I think may be a form of the *Cupressus* of Ballypalady; *Ephedra* also occurs.

Monocotyledons.—The most interesting discovery is that of a *Dioscorea*, identical with one of the most abundant and characteristic leaves at Bournemouth, mentioned sometimes as a *Cinnamon.* It is also met with in the Aquitanian stage of Switzerland, and seems to occupy and denote a very definite zone of age, as it is wanting in the newer English Eocenes.

Dicotyledons.—One nodule contained several leaves of *Platanus*, identical with the Reading form and characteristic in England of the Reading beds, below the London Clay. Another leaf is that known as *Corylus Macquarrii*, from Mull and Greenland. On the other hand, there are remains of the *Cinnamomum lanceolatum* of Bovey and Bournemouth, not hitherto found to the north. Two well-marked fruits are present, which can be referred unhesitatingly to the genera *Alnus* and *Nyssa.* The number of small-leaved dicotyledons referable probably to living genera, give the flora a relatively modern aspect, but many of them, I am convinced, are identical with plants of Greenland and Gelinden. The flora, as a whole, cannot be newer I think than the Middle Eocene, and it is the first important link connecting the Middle Eocene floras of England with those of the far north.

Ardtun Head (Mull).

The specimens exhibited are the result of one day's work. I believe that some of the larger leaves obtained were perfect, but they broke in transit, owing to the brittle nature of the apparently hard matrix. Those previously collected are too fragmentary, and good specimens might easily be obtained by quarrying away the basalt. As the bed is extensive, collections might be made from different spots, and greater variety of plants procured. The leaf-bed is clearly in the track of an ancient river, whose former bed must be exposed in other parts of Mull.

Ferns.—The only one is abundant, but represented by torn fragments, owing to the original tender consistence of the fronds. It is no less plentiful and broken up in some of the Greenland beds, whence it was described by Heer as *Hemitelites.* This was corrected by Professor Newberry to *Onoclea sensibilis*, the only species of a genus now wholly confined to North America. It is unknown from any other locality in Europe. The jointed and striated stems of *Equisetum* are also abundant.

Conifers.—A number of short pieces of branchlets have been found at Mull by Mr. Koch, and at Canna, which cannot be distinguished from the Ballypalady *Cryptomeria.* Linear leaves, possibly of *Sciadopitys* or an *Abies*, are rarer. The specimen originally figured by Edward Forbes as *Taxites Campbelli* still remains unique, though said by Lyell to be the most abundant conifer. It was claimed by Heer as *Sequoia Langsdorfii*, and was one of the three species which led him to consider the deposit to be of Miocene age. It is a coniferous branchlet with distichous foliage, and without definite generic character. Similar foliage from Disco has also been determined to be the same species, but on what basis is not apparent. Plenty of distichous foliage, but of different type, has been found at Mackenzie River and Spitzbergen, but it hardly follows that because these were referable to *Sequoia*, all distichous foliage must be equally so. The genera *Taxus* and *Taxites* have been almost expunged from lists of Tertiary fossils, yet the extreme antiquity of *Taxus* cannot be doubted, and there is no reason to exclude it from Tertiary floras, for it has a wide distribution now and many species. *Sequoia* forms considerable groves, and where met with fossil, occurs in abundance; but the *Yew* is solitary, and while the fruits of the former, if associated, could not escape detection, the berries of the latter might easily be overlooked.

Monocotyledons.—There is the usual proportion of reeds and rushes inseparable from plant-beds formed by river-side.

Dicotyledons.—The upper part of the bed is choked with large leaves of a roughly palmate form, seemingly to the exclusion of all

others, unless the great size of the leaf forms so favourable a cleavage plane that no smaller leaves get exposed. The leaf is cordate, imperfectly lobed, serrate, with much the same venation and texture as the *Lime* or the *Mulberry*. It is so large that no perfect specimen has yet been obtained, though there need be no difficulty in doing so. The leaf varies from imperfectly cordate to trilobed. It is the *Platanites hebridicus* of Edward Forbes, and *Platanus aceroides*, according to Heer, but many species seem included under the latter name, only some among them belonging truly to *Platanus*. The Mull leaf is quite unlike *Platanus*, and of altogether different texture. The prevailing and almost the only leaf in the lower half of the bed is that figured as *Rhamnites* by Forbes. It is an oblong leaf, with simple margin and feather veined, varying in length from 1 to 3 inches, and could hardly be determined without some further clue. There are also leaves known as *Corylus Macquarrii*, also found at Lough Neagh, a leaf like a *Myrtle*, another Greenland leaf known as *Corylus possedentatus*, an ovate leaf, probably *Cornus hyperborea*, with seven mid-ribs, and two or three other indeterminable fragments.

A remarkable, but not singular, fact about these floras is the great differences between the appearance and composition of the several groups of plants from Ballypalady, Glenarm, and Ballintoy. Dismissing the latter, which merely contains small leaves of *MacClintockia* and a hazel-like leaf, we notice that the remaining two present a very strong contrast in point of the relative luxuriance of growth of the plants contained in them. The Glenarm plants were large leafy trees and shrubs, while the Ballypalady leaves are much smaller, and a large proportion of the plants are coniferous. They possess, however, enough species in common to establish their relative synchrony, and they are practically on the same horizon: but these very species have quite a different aspect from the two localities, the *Cryptomeria* fruit and foliage especially being dense and luxuriant from the one, and poor and starved from the other. Moreover, the Pines and Cypress, so abundant at Ballypalady, are completely absent from Glenarm. The differences are in fact just such as a great discrepancy in the altitude of the stations might produce, but in view of the fact that both sets of beds were in all probability deposited in one river channel, and of the horizontality of the Basalt flows, no such considerable difference of level could be possible in so short a distance. The relative luxuriance at Glenarm must therefore have been due to a sheltered aspect, soil, and other favourable conditions, and the possibility of such causes modifying to an extent where resemblance almost ceases, two floras that are contemporaneous, and comprise many of the same species, and within a few miles of each other, teaches what excessive caution we should use in drawing inferences from fossil plants. These three floras, which are below the horizon of the

columnar Basalts, agree in one particular; they are all characterised by the presence of the peculiar triple nerved leaf called *MacClintockia.* The flora of Lough Neagh differs considerably from the others. The plant-beds, instead of being only a few feet thick, exceed 300 feet in thickness, and as only the upper part of the mass is accessible, those collected may be of newer relative age, as their aspect indeed suggests. The most typical of the older Basaltic plants are absent, but some of the minor leaves are common to Ballypalady and also to Mull, and I do not think they can be placed higher in the Tertiary series than Eocene, and they certainly do not appear to me to be Pliocene, as supposed by the Geological Survey of Ireland. The Mull flora, being among the columnar Basalts, is stratigraphically newer than those of Antrim, with the probable exception of the Lough Neagh beds, and *MacClintockia* is absent from it.

We thus see that the three floras of greatest antiquity, though differing widely from each other in other respects, are all characterised by the presence in them in abundance of *MacClintockia*, whilst those which we have stratigraphical evidence for believing to be of newer age are destitute of anything resembling it.

Now *MacClintockia* only occurs in one stratum whose age has been fixed wholly independently of plants. This is the Heersien stage of Gelinden, a stage very low down in the Eocene of Belgium, and older than any Eocene represented in England. Our floras from the Woolwich and the Reading beds are now tolerably well known. They are younger than the Gelinden flora, and contain no *MacClintockia*, just as we have remarked with the younger floras of the Basalts, but the prevailing type in them, a *Platanus*, does occur at Lough Neagh. Here we have fossil evidence of precisely the same nature that we have been accustomed to regard as conclusive when dealing with anything but vegetables—*MacClintockia* characterises the Heersien stage in the only locality in which a flora of that age is known, and it is not met with elsewhere or at any other horizon in Europe. There is not a particle of other positive evidence as to the age of the Antrim Basaltic floras, except that they are posterior to an horizon in the White Chalk, but the physical grounds for assigning a high antiquity to the Basalts are overwhelming. This is not the place to enter into them, and I have recently stated them at some length. The floras cannot be Miocene, for they have not a single plant in them common to any European Miocene deposit, and if Eocene, they must, in the absence of anything contradictory, be placed in the only Eocene horizon in which precisely the same plants are found. The same with the Lough Neagh beds; stratigraphically they appear to be newer than the rest; they contain a particular plant that the others do not, and this plant is eminently characteristic of the Reading series of the Eocene and of no other, for it is absent from the older flora of Gelinden, just as it

is in Antrim, and from all the newer floras of the Eocene. I have chosen the commonest and most striking types to base my argument upon, for the sake of brevity, but I could support them by reference to the other plants which, so far as I have examined them, entirely support them.

Were the belief in the Miocene ages of these beds not so deeply rooted, it would be unnecessary to prove what is self-evident. Where the ages of Tertiary rocks have been decided upon plant evidence alone, we should, I think, accept them for the present with great reserve. *MacClintockia* seems, so far as we know, to be an extinct and striking type which had a definite range in time, but many of the plants associated with it have persisted to the present day almost without modification. Still, their presence in England may be equally indicative of a definite age of rock, though it would not do to extend the use of their presence as standards of antiquity to strata in their own existing habitats. Thus, *Onoclea sensibilis* represents a definite stage in Mull, but as a United States fossil, where it still lives, we cannot say that its occurrence proves strata to be contemporary with those of Mull. So with *Osmunda javanica, Chrysodium aureum, Araucaria Cunninghami, Cryptomeria japonica, Taxodium distichum*, and many others, still living; they mark a definite horizon here, or period at which they flourished, and then disappeared, but in course of their subsequent migrations they may have appeared in other countries at quite different dates, and to draw comparisons as to the relative ages of rocks from their presence in them might be most misleading.

I have not been to Greenland, and I do not therefore know how the plant-beds occur there. Some of the plants are identical with those of the Antrim Basalts, some with those of Mull, and some with plants from Lough Neagh. Whether these are from one horizon or from several I do not know, but they are embedded in different qualities of rock, and presumably therefore come from different levels. Not a shadow of stratigraphical evidence has been brought forward for assigning them to any particular stage of the Tertiaries, but it would seem natural to classify them rather with the most tropical of the Eocene periods, that of the London Clay, than with any other, as their presence so far north, when so high a temperature prevailed in our latitude, presents little or no physical improbability. The London Clay period was preceded in England by a relatively very temperate climate, and the plant-beds of Greenland are preceded by an unfossiliferous zone, marking a period when no floras were in existence so far north. There is already abundant evidence that many plants and animals passed from Europe to America by way of these high latitudes during the Eocene, and evidence of every kind points to some of the plant-beds at least being of early Eocene age. The Icelandic fossil plant-beds I have ascertained to be on an altogether different and far newer

horizon, relative to those of the British Basalts, and their floras do bear a resemblance to some of those said to be Miocene of Central Europe; but as for the Greenland plants brought home by Whymper and by Colomb, they have no Miocene characters whatever, and there should be no hesitation in referring them to the same ages as the Basaltic floras of our own country.

II. "Note on the Later Stages in the Development of *Balanoglossus Kowalevskii* (Agassiz), and on the Affinities of the Enteropneusta." By WILLIAM BATESON, B.A., Scholar of St. John's College, Cambridge. Communicated by Professor MICHAEL FOSTER, Sec. R.S. Received December 4, 1884.

In the "Quart. Journ. Micr. Sci.," vol. xxiv, n.s., p. 208, I described the embryonic stages in the development of a small species of Balanoglossus which is found on the American coast. By a direct embryonic development, this animal reaches the condition in which one pair of gill-slits is present, no "Tornaria" stage being passed through. I have since had an opportunity of observing the further development of this form until it becomes a mature animal possessing from thirteen to forty pairs of gill-slits. The remarks which follow contain a preliminary account of the steps by which this condition is reached.

At the time of hatching, the body of the animal consists of four regions: (1) a conical proboscis bearing at its apex a tuft of long cilia; (2) a very short collar-region; followed by (3) a dilatation of the body, in the dorso-lateral regions of which a pair of gill-openings soon appear. Immediately behind these gill-slits is a transverse ring of cilia, which separates the third region from (4) the anal region. The opening of the mouth is still ventrally directed, and the anus opens at the dorsal side of the posterior surface in the middle line.

The following are the external changes which then occur:—

(1.) The transverse ring of cilia disappears, being absent in larvæ possessing two pairs of gill-slits. The apical tuft of cilia also disappears. The cilia which cover the whole body increase in size, and unicellular glands appear in the skin, especially of the proboscis, giving it a speckled appearance. The tissues become gradually more transparent, presumably from the consumption of the yolk particles with which the cells had previously been filled.

(2.) As the cilia disappear, a remarkable temporary organ appears in the form of a conical process, or tail, arising from the posterior

ventral surface, immediately below the anus. The skin of this organ is wrinkled by five to seven wrinkles, and is full of large glands, probably secreting mucus. It serves as a strong sucker, by which the animal can be kept in position. Its presence is possibly correlated to the fact that at this period of larval life the animals creep up to the surface of the sand, protruding the conical proboscis at the bottom of the shallow pools in which they live. As the heat of the sun beating upon the sand-flats at low tide is very great, it is probably important that the larvæ should not be washed out of the pools by the tide, and so dried up, which is prevented by the presence of this sucker. The whole organ atrophies soon after seven gill-slits are acquired, at which stage the body is long enough to be coiled round foreign bodies.

(3.) As the animal increases in size, the animal becomes more and more flexed upon its ventral surface. This condition is never lost throughout life, the body of the adult being twisted in a right-handed corkscrew spiral, which cannot be completely straightened without stretching the tissues.

(4.) The constriction at the base of the proboscis increases until the stalk in a specimen possessing four gill-slits is not more than $\frac{1}{200}$ inch in thickness.

(5.) In the third region of the body a constriction appears immediately in front of the gill-slits. This constriction is especially developed on the dorso-lateral aspects. It is posteriorly directed, passing from above downwards. The skin of the region thus marked out between the gill-slits and the original collar region becomes thicker, and never acquires the transparency of the rest of the body. At the period at which three gill-slits are present it grows backwards, gradually forming an opercular fold over half the first gill-slit; subsequently in the adult it covers the three anterior pairs of gill-slits. These gills thus open into a small atrial cavity. The skin of the collar becomes filled with mucous glands, and acquires a bright orange colour. The collar is thus made up of two portions of different origin, and, as will be shown, its cavity is lined by mesoblast derived from both the second and also from the third pair of mesoblastic sacs.

(6.) As a result of the deepening of the constriction between the proboscis and collar, and by a forward growth of the lower limb of the latter, the mouth is anteriorly directed when three pairs of gill-slits are present.

(7.) In larvæ with 2—3 pairs of gill-slits, the first appearance of the division of the alimentary canal into three regions may be observed. This division is apparent as soon as the skin becomes transparent, and is very conspicuous owing to the presence of a bright yellow-brown secretion, probably of hepatic character, in the middle or stomach

region, which may thus be distinguished from the pharyngeal region in front and the intestinal tract behind. In the walls of the former the gill-slits are placed. They increase in number from before backwards, throughout larval and adult life. Their openings are at first circular, but afterwards become horse-shoe shaped, owing to the downward growth of a process from the dorsal margin of their openings. This process (the "valve" of Spengel) appears about a week after the first appearance of each gill-slit. By the continual dorso-ventral elongation of the gill-slits, together with an antero-posterior compression, their openings become U-shaped.

The digestive region is yellow-brown in colour. Its walls are thrown into a spiral fold, giving it a sacculated appearance when seen in transparent specimens. The intestinal region is straight, and long cilia may be seen working in its interior, causing an outward current. The anus is now dorsal, and can only be closed by an indrawing of the intestine.

(8.) A large vesicle may be seen pulsating in the base of the proboscis, as described in Tornaria by previous observers. In the dorsal middle line is a longitudinal vessel, which contracts peristaltically, like that of *B. minutus* (Spengel). These contractions appear to have no constant direction. The vascular movements are not correlated to movements of the body. No contractions were seen in the ventral vessel, though their absence was not satisfactorily shown. No further points of importance can be seen in an examination of the larvæ as whole objects.

It has been stated by Spengel with regard to *B. minutus*, that water is taken into the body cavity of the proboscis at the proboscis-pore, and into that of the collar by the two ciliated funnels which open into it. My own observations do not confirm either of these statements; on the contrary, particles of Indian ink or carmine held in suspension in the water cannot be found to enter into either of these cavities, while if placed artificially in them are driven out at all three of these points. On the other hand the particles are carried into the alimentary canal by the ciliary currents, and are more or less expelled thence at the gill-slits. Spengel's statement of the absence of the pore described by Kowalevsky and Agassiz, at the apex of the proboscis, is true for all the species which I have examined.

Internal Structure.

Alimentary Canal.—As mentioned above, the mouth is anteriorly directed. The gill-clefts arise as saccular paired evaginations from the pharynx which come into contact with the skin. The two layers coalesce, become very thin, and then break down, forming openings into the alimentary canal, which are at first circular. In young speci-

mens (1—2 gills) a rod of hypoblast, solid in front, and behind containing a lumen opening into that of the pharynx, is gradually constricted off from the hypoblast in the dorsal middle line of the pharynx. This hypoblastic rod grows forward into the proboscis cavity, pushing in the mesoblastic lining. The lumen opens into the gut throughout life. In *B. Kowalevskii* the opening is slit-like, and extends through the middle third of the collar. A complicated structureless skeleton is secreted by it on its ventral side, which is continued posteriorly into two diverging horns, which lie in the walls of the fore-gut. This skeletal structure in *B. Kowalevskii* differs slightly from that described by Spengel for *B. minutus*. In young larvæ the cells of this hypoblastic rod are columnar, but they gradually become irregular, vacuolated, and enclosed in a sheath, eventually in larvæ with seven or more gill-slits presenting an appearance precisely comparable to that figured by Scott for the notochord of young Lampreys, and by Balfour for that of young Elasmobranchs. It becomes narrowed in the region of the proboscis stalk, where the skeletal structures secreted by it attain their maximum thickness. When isolated it is found to consist of hard cartilaginous tissue. It serves to support the proboscis stalk, and to stiffen the wall of the collar; also as an attachment for the muscles of the collar and proboscis. This is the statement of its function which is also made by Spengel.

From its development, position, histology, and function, it appears to me to be comparable with the notochord of the Chordata, and by this name it will be hereafter alluded to. This view of its homology is supported by the presence in the Enteropneusta of many other structures pointing to vertebrate affinities.

The Nervous System.—The commencing separation of the central nervous system at the time of hatching as a solid cord in the dorsal middle line of the collar extends itself anteriorly as far as the collar-fold, and posteriorly to the region of the first gill-slit. In advanced larvæ (ten gill-slits) its anterior end contains a canal opening to the exterior in the dorsal middle line at the anterior end of the collar-fold. This canal will be spoken of as the neural canal, and its pore as the neural pore. As far as it is possible, in the absence of figures, to follow Spengel's account of the development of this structure in Tornaria, it appears to take place by the invagination of a median furrow in this region, the neural canal being thus present from the beginning. [As Spengel states, in *B. minutus*, &c., the lumen is posteriorly not continuous, but is broken up by strands of nerve cells.] This also holds for the anterior end in *B. Kowalevskii*.

The rest of the nervous system, excepting that at the base of the proboscis, does not appear till much later (three to four gill-slits). Its position has been already described. Its origin in each case is due

to the formation of fibres and occasional ganglion cells from the lowest layer of the skin.

The Body-cavities.—I have already described the formation of the proboscis body-cavity from a single anterior pouch of the archenteron, having two posteriorly directed horns. On the left of the proboscis stalk (one to two gill-slits) an internal thickening of the epiblast arises. A cavity appears in this structure, which becomes open to the exterior. Subsequently it opens internally to the left horn. This opening is the proboscis pore. Spengel states that in *B. Kupfferi* a similar pore is also present on the right side. In connexion with this fact I may mention that in a single specimen of *B. Kowalevskii* (seven gill-slits), which was also otherwise abnormal, I found this double arrangement.

The two collar body-cavities coalesce in the adult on the ventral side, the septum remaining dorsally. Their cavities are prolonged into two horns, which run in the proboscis stalk side by side with the posterior horns of the anterior body-cavities.

Into the collar body-cavity opens the pair of ciliated funnels leading to the exterior, mentioned by Spengel. He describes them without a figure as arising from the anterior gill-pouch. In *B. Kowalevskii* they arise as thickenings of the mesoblast, lining the inner fold of the posterior limb of the collar. These thickenings become hollow and strongly ciliated, opening with the first gill-slit into the atrial cavity formed by the collar fold. In *B. minutus* this atrial cavity is reduced to a minimum; the posterior skin of the collar runs, therefore, directly into the skin of the first gill-slit. It appears that these funnels are of an excretory character. This view is supported by the fact that Hatschek has recently mentioned the presence of an excretory tube opening with the first gill-slit of Amphioxus on the left side. The posterior pair of mesoblastic pouches remain separate throughout life, lining the whole body-cavity posterior to the collar. They send forward a pair of horns between the notochord and the neural cord as far as the proboscis pore.

The first *blood-vessel* arises as a splitting in the septum formed by the adjacent walls of these two horns, and is continued forwards with their growth. This remains throughout life as the dorsal blood-vessel. The ventral blood-vessel is formed as a split in the ventral septum between the third pair of body-cavities.

The dorsal vessel is continuous with the cavity which is correctly described by Spengel as immediately overlying the notochord in the proboscis. This cavity is filled with coagulum, its walls are muscular, and it would seem to me to be the true heart. The space overlying it, which is called by Spengel "the heart," contains, as he has described, a variable quantity of loose cellular tissue. This may possibly be of a glandular character; it is continuous with that of the

glandular body which is attached to the end of the notochord. As far as could be observed through the body wall of the living animal, the pulsations in this region are those of the tube lying immediately upon the notochord. Moreover, this structure gives off all the blood-vessels of the body, both according to Spengel and from my own observations, while none of them are supposed to arise from the glandular portion, which has besides no distinct muscular wall.

The "glandular body" lying at the sides of these structures is in appearance closely comparable to the so-called "heart" of Asterias, being perforated by a network of blood-vessels (Spengel).

The posterior body-cavities are prolonged into the cavity of the temporary tail, remaining separated by the vertical septum.

It may be desirable to discuss briefly the new light which these facts throw upon the affinities of the group Enteropneusta.

In 1881 Metschnikoff published a detailed comparison of Balanoglossus with the Echinoderms, comparing Tornaria with Bipinnaria, showing that the resemblance is close, and concluding with the suggestion that Balanoglossus should be included among the Echinodermata in a separate division, "Bilateralia." The branchial structures he compared to the openings from the body cavities of Echinoderms. This view, as thus expressed, receives no support from further observations, and would now appear to be untenable.

As mentioned above, all the Enteropneusta possess a supporting structure which is comparable with the notochord in every way, except in extent and in the persistence of its connexion with the alimentary canal. Its resemblance to that of Amphioxus is especially striking, for in Amphioxus the notochord projects a long way in front of the mouth. It moreover possesses gill-slits which are not only without parallel, except among the Chordata, but also in structure, position, and development, agree exactly with those of Amphioxus, in which the slits acquire the same U-shaped form.

The agreement in the position of the blood-vessels and skeleton of the gill-bars is also very close. The fact of their gradual increase in number from before backwards throughout life is another common feature.

The position and mode of origin of the central nervous system is also similar in both forms; the invagination of the dorsal cord in Balanoglossus being, however, only partial, while that of Amphioxus is complete.

The mesoblastic pouches suggest the same resemblance, differing only from those of Amphioxus in number, being one median and four lateral, while those of Amphioxus are one median and twenty-eight lateral. As I have already pointed out, the fate of this anterior pouch is in the two animals closely similar. In both it is divided into two as the notochord grows forward. In Amphioxus the division is complete,

while in Balanoglossus it is partial. In both, the backwardly-projecting horn upon the left side becomes lined by ciliated columnar cells and opens to the exterior. Moreover, in both animals this opening has a definite relation to the nervous system. In Amphioxus it becomes the "olfactory" pit (Hatschek), while in Balanoglossus it is surrounded by a mass of nervous tissue. Finally, the collar folds, especially of *B. Kowalevskii*, would appear to be comparable with the commencing atrial folds of Amphioxus, for the most anterior gill-slits open into the cavity which is thus enclosed.

The pair of ciliated funnels opening from the collar body cavities to the atrium has been compared above to the excretory tube mentioned by Hatschek in a similar position in Amphioxus.

A pair of tubes has been described by Lankester in Amphioxus opening into the back of the atrial cavity, communicating with the dorsal body cavities. It may be remarked that if the collar fold of *B. Kowalevskii* were prolonged backwards, as the atrial folds are in Amphioxus, the two collar funnels would then be carried backwards, and have a relation similar to that of these tubes which, as suggested by Lankester, may be excretory.

To recapitulate: Striking resemblances to the Chordata and especially to the Cephalochord type are to be found in the following structures:—

(1.) The notochord.

(2.) The gills and branchial skeleton and blood supply.

(3.) The central nervous system.

(4.) The origin of the mesoblast.

(5.) The peculiar fate and remarkable asymmetry of the anterior pouch.

(6.) The atria.

(7.) The excretory funnels.

In each of these cases, excepting that of the branchial structures and the excretory funnels, the condition is that which would be produced by a partial or arrested development of the corresponding structure in Amphioxus.

The above considerations appear to justify us in including the Enteropneusta among the Chordata. I would, therefore, tentatively suggest the following table:—

CHORDATA:—Hemichordata (Enteropneusta).
Urochorda (Ascidians).
Cephalochorda (Amphioxus).
Vertebrata.

It is not now proposed to enter into a more detailed discussion of the morphology of the group, or of the light which an acceptance of this suggestion throws on the origin of the Chordata. A fuller examina-

tion of these points, together with a detailed account of the anatomy and development, is reserved for a subsequent occasion.

It may, nevertheless, be advisable to point out that since, according to Spengel, the tissue of the "water vessel" of Tornaria forms the lining of the proboscis cavity of *B. minutus*, this "water vessel" is therefore the same structure as the anterior body cavity in the form just described. If then the "water vessel" of Tornaria is comparable to the "water vessel" of Bipinnaria, which has a similarly asymmetrical development upon the left hand side of the body, which view has been held by all previous observers, it would therefore appear to follow that the water vessel of Bipinnaria is *primâ facie* comparable with the asymmetrical anterior body cavity of Amphioxus.

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III. "On Reflex Excitation of the Cardiac Nerves in Fishes." By J. A. McWilliam, M.D., Demonstrator of Physiology, University College, London. From the Physiological Laboratory, University College. Communicated by E. A. Schäfer, F.R.S. Received December 4, 1884.

Cardiac inhibition of a very profound character can be reflexly induced in the eel with a facility that is quite astounding.

Slight stimulation of either gill brings about an immediate arrest of the heart's action, an arrest that may last for several minutes provided the stimulation be continued. Gentle friction or pressure very readily leads to the same result; indeed, a mere touch is sometimes sufficient. A weak interrupted current is likewise effective; also a thermal or chemical stimulus. If water be injected through the gill apertures at considerable pressure so as to distend the branchial chamber, inhibition of the heart's action is similarly induced.

Stimulation of the fifth branchial arch (which carries teeth but no gill) has the same effect.

The branchial nerves are the paths that conduct the afferent impulse to the medulla, whence it passes as an inhibitory impulse down the vagus nerve and along the cardiac branch to the heart. Stimulation of the central end of a cut branchial nerve leads to the same result as stimulation of the gill, and if one vagus nerve be cut, stimulation of a branchial nerve on the same side can still arrest the heart, provided the vagus nerve on the other side be intact. Section of all the branchial nerves obviates the occurrence of cardiac standstill as a result of gill stimulation, and, of course, destruction of the medulla or section of both vagi has a similar effect.

The administration of ether or chloroform does not suspend the operation of this mode of reflex inhibition, provided the amount of the anæsthetic be not so great as to arrest all manifestations of activity in the medulla, *e.g.*, the respiratory acts. Neither removal of the various parts of the brain above the medulla, nor section of the spinal cord immediately below the medulla, prevents the occurrence of cardiac arrest.

This phenomenon of reflex cardiac inhibition as a result of gill stimulation is one that occurs with almost perfect constancy, and usually with a readiness and profoundness that are very striking. It occurs in the uninjured animal, as may be seen by watching the area of cardiac pulsation on the ventral aspect of the body, while the gill is stimulated by pressure or in any other way. The recommencement of the heart's action is often delayed for a considerable time (half a minute or longer) after strong stimulation of the gill has been dis-

continued. One or more respiratory movements are often excited at the same time that cardiac arrest is brought about. Stimulation of the gill apertures and of the internal surface of the branchial chamber also gives rise to cardiac inhibition, though in a much less marked degree than does stimulation of the gills.

The phenomenon of cardiac inhibition as a result of gill stimulation is not confined to the eel. I have found it to be well marked in various other fishes—carp, perch, rudd, and others.

Stimulation of the skin of the head is also effective in bringing about an arrest of the heart's action. The application of pressure or slight friction, or a weak interrupted current, or a thermal stimulus, leads readily to this result. If the skin be removed, slight stimulation ceases to have any effect unless pressure be applied over the branchial chamber—an inhibitory effect is then obtained by the gills being affected. *Severe* injury of any part of the head after removal of the skin may still (apart from any influence upon the gills) bring about cardiac inhibition. Stimulation of the fifth nerve is followed by a similar result. The administration of ether or chloroform seems as a rule to prevent the occurrence of cardiac arrest as a result of *slight* stimulation of the skin of the head; this occurs when the quantity of ether that has been used is quite insufficient to suspend the activity of the medulla, or to prevent the arrest of the heart which follows gill stimulation.

Gentle compression of the animal's tail between the fingers is generally followed by standstill of the heart. So also is the application of electrical or chemical stimulation to the same part. The inhibitory result is obtained by irritation of the integument covering the caudal fin as well as of that covering the muscular part of the tail. If the skin be removed gentle mechanical stimulation ceases to have any effect; electrical stimulation is apt to spread to the spinal cord. The area of skin over which stimulation is effective generally extends from the tip of the tail forwards about one-eighth of the distance to the vent.

Muscular movements along the animal's body are induced at the same time as the cardiac standstill.

The path along which the afferent impulses pass to the medulla is the spinal cord. Section of the cord above the point of stimulation prevents the occurrence of a cardiac inhibitory result.

Direct stimulation of the spinal cord is a very powerful means of arresting the heart's action, and the same result occurs at whatever part the cord is stimulated. The cardiac effect is accompanied by vigorous contraction of the muscles generally, including those of the pectoral fins. But when an interrupted current is employed to stimulate the cord, the question may be raised whether some escape of the current to the neighbouring structures may not occur, *e.g.*,

whether the current may not (if a strong current be used) spread to the sympathetic nerves lying along the aorta on the ventral aspect of the vertebral column. Any such source of error can be avoided by employing mechanical stimulation of the cord by means of a wire passed some distance up the spinal canal, so as to destroy the cord at that part.

Moreover, if an interrupted current of the same strength as before be applied to the empty spinal canal at the point where the cord was formerly stimulated, it is found to cause no inhibition at all, though it is quite as free as before to pass into the sympathetic nerves and other neighbouring structures. It is clear then that the interrupted current which caused inhibition when applied to the cut end of the spinal cord caused inhibition by stimulating the cord itself, and not by its effect upon any neighbouring structures into which it might have spread.

When direct stimulation of the cord is employed as a means of inducing reflex cardiac inhibition, the accompanying movements of the skeletal muscles occur so spasmodically that it is impossible to observe any order of succession in their generation.

But when the animal appears to be in a sluggish and depressed condition, stimulation of the tail is often followed by a struggling movement which begins in the region of the stimulated point, and slowly passes along the body towards the head. When this wave of movement passing along from the tail reaches no further than the middle of the animal's length, no cardiac inhibition occurs, but when it traverses the whole length of the body, along to the head, cardiac inhibition occurs, and removal of the brain above the medulla does not obviate this result. The wave of muscular movement passing along the body from tail to head (as a result of stimulation of the tail) is not simply a contraction wave propagated by the muscular tissues; it indicates a wave of activity passing over the motor cells of the spinal cord. Section of the spinal cord at once arrests the transmission of the movement in question. It would seem that when this phase of activity passing along the cord reaches the medulla, the vagus centre is thrown into action together with the neighbouring motor centres, *e.g.*, the respiratory centre, the centre for movement of the pectoral fins, &c. It must be remembered that in these experiments the animal is made fast on a board. In such circumstances, the struggling movements possibly attain an intensity much greater than is commonly the case in the normal condition of the animal.

When the wave of action passing along the spinal cord fails to reach the medulla, the vagus centre would seem to be unaffected, since no change in the heart's action is discernible. And when irritation of the tail fails to produce any reflex movements, it also fails to bring about cardiac arrest. The result of the application of a stimulus

to the tail is not always the same. After the tail has been repeatedly stimulated, with the effect of causing reflex muscular contraction and cardiac inhibition, a phase (of exhaustion) often occurs, during which stimulation fails to bring about either reflex movements or cardiac arrest.

The effects of the administration of strychnine (by hypodermic injection) are in accordance with the phenomena already described. After strychnine poisoning convulsive movements occur, as in the frog, in response to stimuli of a very trivial character. But in the eel, the advent of each convulsive attack is accompanied by a temporary arrest of the heart's action. The vagus centre seems in this instance also to be thrown into action in company with the ordinary motor centres.

Again, when experimenting with an eel, struggling movements of the animal occasionally occur without any apparent cause. These movements, if confined to the tail part, are accompanied by no perceptible change in the heart's action, but when they involve the whole length of the animal or the part next the head, cardiac arrest occurs at the same time—just as cardiac arrest accompanies the struggling movements resulting from stimulation of the skin of the head or tail. When the motor centres of the cord and medulla (including the respiratory centre) are paralysed in consequence of a prolonged administration of ether or chloroform, the vagus centre also seems to be *hors de combat*. Reflex inhibition of the heart does not then occur as a result of the stimulation of any part, though direct stimulation of the vagus nerve readily leads to prolonged inhibition.

It will thus be seen that stimulation of the skin of the head and of the tail of the eel is peculiarly effective in causing reflex inhibition of the heart's action. The skin of the head and tail are shown to be possessed of a specially high sensibility by results other than cardiac arrest. For if a vigorous uninjured eel when lying quiescent on a board be lightly touched on the head with the finger, reflex movements of a more or less extensive and complex character are immediately manifested. Similar results are obtained by holding between the fingers or very gently compressing the tail; the resulting movements are sometimes confined to the caudal region, while they at other times affect the whole length of the animal. But when the intermediate tract of the eel's body is tested in the same way, it is found that no visible effects of any kind follow the application of gentle pressure to the skin. The skin of the animal's body seems to be quite irresponsive to stimuli which produce striking effects when applied to the head or tail.

It would seem then that cardiac arrest is most readily excited by stimulation of those cutaneous surfaces which are prone to yield reflex movements as a result of similar stimulation. In other words, stimu-

lation of those regions of the skin which most readily lead to reflex excitement of the spinal and medullary motor centres also leads most readily to excitement of the vagus centre.

Stimulation of the parietal peritoneum very readily gives rise to reflex cardiac arrest.

Tapping the ventral aspect of the abdomen of an intact eel is commonly followed by more or less marked slowing of the heart's action. And when the abdominal cavity is being laid open, the heart generally stands still for a considerable length of time. When this standstill has passed away, the introduction of a finger-tip into the abdominal cavity so as to come into contact with the parietal peritoneum is frequently followed by a temporary cardiac inhibition. If the abdominal parietes be now fixed apart so as to expose the cavity fully, it can be readily observed that stimulation of the parietal portion of the peritoneum is a very effective cause of reflex cardiac arrest. Application of slight friction, a weak interrupted current, or a hot wire to the internal surface of the abdominal parietes, almost invariably brings about cardiac inhibition in a very marked degree. Contraction of the neighbouring trunk muscles commonly occurs at the same time; such contraction at times involves the muscles along the whole length of the body. These muscular contractions (accompanying the reflex cardiac arrest) occur on both sides of the body, and are evidently a reflex effect of the peritoneal stimulation. A very weak interrupted current is sufficient to bring about the results mentioned: such a current as is obtained from the secondary coil (of a du Bois-Reymond's induction machine, with the Helmholtz modification) at a distance of 11 cm. from the position where it covers the primary coil completely, whilst the battery used is a single Daniell's element. The effect on the heart's action caused by stimulation of the parietal peritoneum seems to occur more readily in response to a weak interrupted current than to slight mechanical stimulation.

If the parietal peritoneum be stripped off from the subjacent tissues at any part where reflex inhibition is found to be readily excited, the application of the electric current to the same spot—now denuded of its peritoneal covering—fails to exert any influence on the heart's action, unless the current spread to one of the spinal nerves. But if the electrodes be moved to another part of the abdominal parietes—to a part where the peritoneal layer is still intact—the same cardio-inhibitory phenomena as before can be readily observed. Mechanical and thermal stimulation give similar results.

The afferent impulses generated by stimulation of the parietal peritoneum seem to pass to the medullary vagus centre chiefly through the spinal cord. Section of the upper part of the cord obviates the cardio-inhibitory effect of peritoneal stimulation. Section of the vagus nerves below the origin of the cardiac branches does not

prevent the inhibitory result. Curiously enough, stimulation of the visceral peritoneum has an entirely different and indeed an entirely negative result; it is followed by no perceptible change in the heart's action. Even strong interrupted currents, and strong mechanical, chemical, and thermal stimuli applied to the visceral peritoneum, seem to be quite without effect on the cardiac beat. Powerful electrical stimulation of the whole thickness of the walls of the stomach and intestine has no perceptible effect on the heart. Pinching, crushing, and tearing of these parts are equally ineffective; and so are chemical and thermal irritants.

Here I must advert to an experiment described by Marshall Hall in "Todd's Cyclopædia of Anatomy and Physiology" (article *Heart*). He states that when the stomach of an eel was crushed by a violent blow with a hammer, the heart stood still for a considerable length of time, *and this even though the animal's brain and spinal cord had been previously destroyed.* From this he argues that the cardiac action can be inhibited by impulses originating in the stomach, and acting upon the heart without the mediation of the cerebro-spinal system at all.

Now I have many times violently crushed the stomach of an eel by means of large and strong pliers, and I have never observed any cardiac arrest resulting therefrom. This statement holds both in the case of eels that have had their brain and spinal cord intact, and in eels that have had these organs destroyed. It would seem therefore that the cardiac inhibition observed by Marshall Hall was in all probability *not* simply due to the crushing of the stomach—to which he ascribes it. It must have occurred as a consequence of some effect of the blow other than the crushing of the stomach. Indeed, his method of experiment was such as to render possible the agency of many causes other than those strictly dependent on the injury of the stomach; his method rendered possible the excitation of many nerve impulses besides those generated by the stomach injury. For a severe blow delivered with a hammer upon the intact stomach of an eel would necessarily injure various other structures, and would at the same time give rise to a considerable amount of jar. Injury of such structures adjacent to the stomach as might be effected by a hammer blow, cannot be put down as the efficient cause of the reflex cardiac arrest, since powerful stimulation of these parts (applied so as to obviate the accompaniment of jar) gives rise to no effect at all on the heart's action, provided the brain and spinal cord have been previously destroyed. After destruction of these organs I have, however, in several instances observed a well-marked inhibition of the heart's action as a result of sudden jarring of the animal generally—such jarring as may be caused by forcibly striking the board on which the eel rests. This result does not occur in all cases, but in some instances I have seen it in unmistakable form. Such a jarring was

probably caused by Marshall Hall's blow upon the stomach, and to such jarring may, it seems to me, with most probability, be attributed the cardio-inhibitory phenomenon which he describes.

Stimulation of the air-bladder and the other abdominal organs appears to be without effect on the heart's action. The abdominal organs seem, in fact, to be the only parts of the animal from which cardiac inhibition cannot by powerful stimulation be excited. And similarly with regard to reflex movements, little or no effect is manifested in response to the most powerful stimulation of the abdominal organs.

Stimulation of the abdominal sympathetic nerves seems to have no influence on the cardiac action.

Stimulation of the central end of the right or left vagus nerve below the origin of the cardiac branch causes well-marked reflex inhibition, provided the nerve be stimulated in that part of its course where it lies along the gullet. Stimulation of either vagus nerve below the gullet, *i.e.*, after the nerve has passed from the gullet on to the stomach, seems to have no cardio-inhibitory effect. Stimulation of the inner and outer surfaces of the gullet itself appears to be without effect on the heart; whilst the application of a very weak current to the vagus nerve lying along its outer surface, most readily brings about a prolonged arrest of the cardiac action.

Stimulation of the first spinal nerve, or any of its branches, gives rise to marked inhibition of the heart. Many of the branches of this nerve can be very easily isolated, as they pass to the muscles on the ventral aspect of the body anterior to the pectoral fins. Stimulation of the central end of any of the spinal nerves is often followed by cardiac arrest.

Irritation of the mucous membrane of the mouth seems in most instances to lead to cardiac arrest. This can easily be shown by introducing any pointed instrument into the mouth and scratching the mucous membrane. General muscular movements are usually induced at the same time as the cardiac inhibition.

Irritation of the pharynx seems to be still more effective as regards its influence in suspending the cardiac action.

Severe injury of almost any part of the animal—with the exception of the abdominal organs—is usually followed by a more or less prolonged standstill of the heart. This result is generally accompanied by more or less extensive and continued movements of the skeletal muscles. Injury of the pectoral fins seems in many instances to be more effective in causing reflex inhibition than does injury of the body generally, excepting of course the specially effective parts (*i.e.*, head, tail, parietal peritoneum, and gills) already mentioned.

I have on many occasions investigated the effect of stimulation of the great lateral nerve upon the cardiac movements. This nerve

originates from the bunch of nerves which make up the vagus. It separates from the other portions of the vagus near the skull; it then passes along the lateral surface of the animal, pursuing the same general course as the lateral line, though for the greater part of its course at a considerable depth from the surface, embedded among the muscles. In the eel it does not extend to the tail, as it does in some fishes (*e.g.*, carp); it terminates at a variable point, sometimes about the junction of the caudal third with the rest of the body, sometimes in the middle third of the animal's length.

Section of this nerve is followed by no visible effect. And stimulation of its central end seems to exert no marked influence on the cardiac action.

The sudden placing of an eel in the vertical position with the head downwards, sometimes leads to a temporary cardiac arrest, provided the circulation be intact. Movements of the animal occur at the same time as the inhibition of the heart, and when no such movements result from the placing of the animal in the position referred to, the cardiac inhibition also fails to occur. And even in the cases where cardiac inhibition (accompanied by movements of the animal) does occur, the inhibition is usually a very temporary one. The heart is not kept at a slow rate during the whole time that the vertical position is maintained; in fact, there is generally, after a little time, a slight acceleration of the heart's rhythm beyond what was seen when the animal was in the horizontal position. In the vertical position (with the head downwards) the heart is much distended with blood, and fails to empty itself at each beat. And section of both vagi does not seem to have any effect upon the gorged condition or slightly accelerated rate which the heart exhibits while the above-mentioned position of the animal is maintained.

When the eel is held in a vertical position with the head upwards, the heart becomes small, pale, and empty, or almost empty. Its rate of action remains unchanged, or after a little time becomes somewhat slower than in the horizontal position; the latter result (a somewhat slowed rate) seems to be the one that more commonly occurs.

Reflex arrest of the heart—brought about in any of the ways mentioned—does not depend upon alterations in blood-pressure. It occurs after section of all the large blood-vessels.

The eel's brain can be readily exposed by removing the cranial roof by means of a pair of strong scissors or a bone forceps. The effects of stimulation and removal of the various parts can then be tested.

Stimulation of the cerebral hemispheres has no apparent effect either with regard to the condition of the heart or of the skeletal muscles. And removal of these parts seems to have no marked effect.

Stimulation of the optic lobes leads to very striking results; the

heart stands still, while strong contraction of the skeletal muscles occurs. These results are got by the application of very weak interrupted currents; and also by mechanical stimulation. The effects produced do not seem to be due to a spreading of the stimulating current to the medulla; the currents used are extremely weak, and, moreover, stimulation of the cerebellum (which lies much nearer the medulla than do the optic lobes) gives no effect at all or only a very trivial effect compared to that obtained from the optic lobes. If the interrupted current applied to the optic lobes spread to the medulla, it ought surely to spread as readily or indeed much more readily to the medulla when applied to the cerebellum.

Removal of the optic lobes seems to lead to a change in the readiness with which stimulation of certain parts leads to reflex cardiac inhibition. The application of slight friction or weak electrical currents to the skin of the head seems (after removal of the optic lobes) to have no effect on the heart's action; neither does slight irritation of the mucous membrane of the mouth, nor irritation of the gill apertures. Very severe injury of any of these parts may still, however, lead to cardiac inhibition.

Removal of the optic lobes also seems in many instances to lead to an acceleration of the rate of the heart's action.

Stimulation of the medulla is highly effective in causing prolonged suspension of the cardiac action. A rapidly interrupted current is not essential; a current with interruptions at the rate of two or three per second is effective. Removal of the medulla abolishes all the phenomena of reflex cardiac inhibition.

After removal or destruction of the medulla, the heart rhythm is generally accelerated to a considerable extent. A similar change in the rate of cardiac action can usually be observed as a consequence of section of the vagi. It would seem then that during the time the animal is experimented on with intact medulla and vagi, the heart's action is under a continued controlling influence which renders its rhythm slower than it would be were it not for this controlling influence exerted by the medulla upon the heart through the medium of the vagi nerves. And this controlling influence is manifested when the eel is left uninterfered with for a considerable time, and when the animal appears to be in a perfectly quiescent state. However, the means which are employed to fix the animal, and the incisions which have been made to expose the heart and brain, may be sources of irritation which by sending afferent impulses to the medulla, keep the vagus centre in a state of slight but continued activity. And if extensive injuries have been inflicted, *e.g.*, if the animal is cut across at the middle of its length, the continued controlling influence exerted by the central nervous organs on the heart is, as a rule, decidedly more marked. This controlling influence is much more

strongly evidenced when the optic lobes and medulla are both intact than when the former parts (the optic lobes) have been removed.

That such a controlling influence is at times present in a very marked degree, is evidenced by the fact that section of the vagi sometimes leads to a very great change in the rapidity of the heart's action. The number of beats occasionally increases from a rate of 15 per minute before section of the vagi, to a rate of 40 per minute after section of the vagi. Changes in the degree of continued controlling influence exerted by the central nervous system upon the heart seem to play a very important part with regard to the after effects which are manifested as consequences of reflex arrest of the heart. When the cardiac action recommences after reflex inhibition the beats are at first slow, but soon increase in rate and frequently attain for a time a much greater rapidity of rhythm than was manifested before the reflex inhibition occurred. At the same time a slight increase of the force of the auricular beats may occur. These changes in rate and force seem to be in very many cases due for the most part to a diminution of the constant controlling influence which acted upon the heart before the inhibition was caused. It would seem that strong action of the medullary inhibitory centre (exerted by afferent impulses) and leading to cardiac standstill, is followed by a phase of markedly diminished activity of that centre—a diminution which involves a relaxing of the control exerted upon the heart, and a consequent increase in the rate of the heart's beat and occasionally also of the auricular force of contraction. That such a diminution of the medullary controlling influence is frequently the main cause of the after-acceleration following reflex inhibition, is supported by a number of considerations; among others, by the fact that in many instances where such after-acceleration is well marked as a result of reflex inhibition, no after-acceleration at all occurs after inhibition from direct stimulation of the vagi or of the sinus.

The state of excitement into which the medullary vagus centre is thrown by strong afferent impulses (from the gills and other parts) seems to persist for a considerable time after the stimulation (of the gills, &c.) is discontinued. For the heart frequently remains quiescent for half a minute or more after gill stimulation is discontinued; whereas after direct stimulation of the vagus nerve the heart recommences very shortly after the stimulation is discontinued. Free administration of ether or chloroform leads to complete loss of reflex inhibition as well as of reflex action generally. Cardiac arrest can still be readily observed as a result of direct stimulation of the vagus nerve. A smaller amount of these anæsthetics induces a condition much resembling that seen after removal of the optic lobes. There are no voluntary movements. Reflex action upon the heart or skeletal muscles cannot be excited by *very slight*

mechanical or electrical stimulation of the skin of the head, or by *very slight* stimulation of the gill apertures or of the mucous membrane of the mouth. Weak irritation, which formerly produced striking results, now seems to have no effect on these parts. Cardiac inhibition and reflex contractions of the skeletal muscles may still be excited by *severe* injury of the parts mentioned, or by ordinary stimulation of the gills, tail, parietal peritoneum, first spinal nerve, or trunk of the vagus nerve where it lies on the gullet.

When the heart has been acting very slowly (in consequence of the controlling influence of the central nervous system) prior to the administration of ether, the rate of action usually increases to a very marked extent even when the amount of ether used has not yet been enough to paralyse the reflex activity of the medulla, but when it has induced a condition similar to that seen on removal of the optic lobes.

It will be seen from the experiments described, that reflex inhibition of the eel's heart is often excited by peripheral stimulation which leads at the same time to reflex action of the skeletal muscles. The vagus medullary centre is often thrown into a state of activity by impulses which at the same time set into activity the motor centres of the medulla and cord generally. Such results may be brought about by stimulation of the skin of the head or tail, by stimulation of the gill apertures, or of the mucous membrane of the mouth, and by severe injury of any part of the body excepting the abdominal organs.

On the other hand, stimulation of certain parts can lead to cardiac inhibition without any other indications of reflex action. The medullary vagus centre may as a result of peripheral stimulation be thrown into a state of excitement without the participation of any of the motor centres of the medulla or cord; sometimes with the participation of one or more of the neighbouring medullary centres (*e.g.*, the respiratory). Such results are commonly obtained by stimulation of the gills, of the fifth bronchial arch, of the first spinal nerve, or of the vagus nerve as it passes along the œsophagus below the heart. Stimulation of these parts frequently leads to prolonged inhibition of the heart without the occurrence of any other indication of reflex action.

(In the case of all the animals experimented on, the brain was destroyed or anæsthetics were administered before any operative proceedings were resorted to.)

IV. "The Influence of Stress and Strain on the Physical Properties of Matter. Part I. Moduli of Elasticity—*continued.* The Viscosity of Metals." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received December 9, 1884.

(Abstract.)

After a short account of the researches of Sir William Thomson and Professor G. Wiedemann, on the loss of energy of a wire when vibrating torsionally, the author proceeds to describe his own experiments on the same subject. The wire under examination was clamped at one end into a stout brass block, which was secured to the extremity of a strong iron bracket projecting from a wall. A wooden box nearly 600 cm. in length and 12 cm. square inside, protected the vertically suspended wire from currents of air. The box rested upon another, which measured about 40 cm. each way, provided with a glass window in the front, and a door at the side, which latter was opened only when it was necessary to make a readjustment of a vibrator attached to the lower extremity of the wire, and capable of moving freely inside the box. The bar of the vibrator was either clamped or soldered to the lower extremity of the wire, and on it were suspended two cylinders of equal mass and dimensions; by sliding the cylinders backwards or forwards on the bar the moment of inertia of the whole vibrator could be altered without changing the mass. The torsional vibrations of the wire were observed by the aid of mirror, scale, and lamp, so that, as the length of the wire was upwards of 600 cm., and the diameter rather less than 1 mm., very small molecular displacements were produced when the wire was vibrating.

It was found necessary to allow the wire to rest, after the adjustments had been made, for a length of time varying with the nature of the metal from one day to several weeks; as though great care was taken in arranging the wire, it was impossible in many cases to avoid imparting subpermanent torsion, which gradually came out under the influence of rest and repeated oscillation.

Very great care was taken both in starting the vibrator and in taking the observations, the results of which showed that the diminution of amplitude could in most cases be determined with considerable accuracy.

The various causes of the loss of energy of a vibrating wire are pointed out in the paper, and subsequent experiments proved that all of these are practically neglectable, except the resistance of the air and the internal molecular friction of the metal. A full account is

given of the method of eliminating from the results the effect of the resistance of the air, which with some metals plays a very unimportant part in diminishing the amplitude as compared with the internal friction, but with others, especially under certain conditions, is to be credited with almost the entire loss of energy.

A mathematical investigation of the loss of energy which would be experienced by a wire vibrating under the influence of torsional elasticity, if the internal friction of solids were like that of fluids, proved with reference to the proportionate diminution of amplitude (*a*) that it should be independent of the amplitude; (*b*) that it should vary inversely with the vibration-period. From the experiments it followed that though the condition (*a*) was satisfied, (*b*) was not, so that when the moment of inertia of the vibrator was altered without any change of mass, instead of the proportionate diminution of amplitude being in the inverse ratio of the vibration-period, it seemed to be to a considerable extent independent of the period.

"The fatigue of elasticity" (a term first used by Sir William Thomson), according to which a wire which had been kept vibrating for several hours or days through a certain range, came to rest much more quickly when left to itself than when set in vibration after it had been at rest for several days, was next the subject of investigation, and it was found that this elastic fatigue never showed itself when the wire, whatever might be the metal of which it was composed, was vibrated through amplitudes sufficiently within the limits of elasticity, but, on the contrary, repeated oscillation was in this case always attended with *diminution* of loss of energy, the diminution being subpermanent. No trace of elastic fatigue was discernible in the case of most of the metals examined so long as the vibrations did not exceed in amplitude the limits of the scale, but with nickel it was necessary to confine the amplitude to 100 scale-divisions, in order to avoid "fatigue of elasticity."

Moderate permanent extension was found to diminish the loss of energy of a copper wire and to increase that of an iron wire, provided a rest of one day was allowed; *recent* permanent extension *increased* in both cases the loss of energy. The difference between copper and iron in the above respect is no doubt due to the greater "coercive force" of the latter. Moderate permanent torsion had a much greater effect than permanent extension in increasing the loss of energy of an iron wire.

An examination of the effect of passing an electric current of from ·1 to ·3 ampère through a vibrating iron, nickel, or tin wire ended in proving that such a current, though capable of producing sensible circular magnetisation of the first two metals, had no sensible effect on the loss of energy of any of them.

During the investigation it was thought advisable to make experi-

ments on the loss of energy of a vibrating magnet, and the following facts were elicited:—

(*a.*) The loss of energy of a vibrating magnet, like that of a vibrating iron wire, is lessened by repeated oscillations, and, after the *first* adjustment, or after a slight jar, is lessened by rest only. Repeated oscillation produces a subpermanent diminution of loss of energy.

(*b.*) The diminution of amplitude due to magnetic causes only is like that due to the resistance of the air, and follows the same law, namely, that for different vibration-periods the diminution is inversely as the period, and therefore is in this respect quite unlike the diminution of amplitude resulting from internal friction.

During the whole of the experiments a most careful watch was kept upon any effect which change of temperature might produce in the loss of energy, and it was discovered that whilst with wires of tin, lead, aluminium, silver, platinum, nickel, *unannealed* piano-steel, zinc, copper, brass, German silver, and platinum-silver the loss of energy became greater when the temperature was raised; with iron, on the contrary, the loss of energy was diminished by the same cause.*

The marked difference between the effects produced on the loss of energy of annealed iron and of the other metals, by rise of temperature, caused an extended investigation to be made with annealed iron wire with the following results:—

(*a.*) The loss of energy of a torsionally vibrating iron wire is *permanently* diminished to a very large extent by repeated heating to 100° C. and cooling combined with long rest.

(*b.*) The loss of energy is very considerably diminished temporarily by rise of temperature, the value of the logarithmic decrement at the temperature 0° C. being about *twice* as great as the value at 100° C. The loss of energy at the temperature of 100° C. of an iron wire which has been repeatedly heated to 100° C. and cooled to the ordinary temperature of the room is so small that it may be *almost entirely* accounted for by the resistance of the air.

An examination was also made of the effect of change of temperature on the torsional rigidity of the metal, and it was found that this change could be represented by the formula—

$$r_t = r_0(1 - \cdot0001443t - \cdot0000015804t^2),$$

where r_t and r_0 represented the torsional rigidity at $t°$ C. and 0° C. respectively. Both from the observed torsional rigidity and from that calculated from the formula, it was found that the decrease of the torsional rigidity caused by rise of temperature from 0° C. to 100° C. is only about *half* of that got by Kohlrausch when making a similar investigation. The great discrepancy above alluded to can only have arisen from the different treatment of the iron previously to the actual

* Probably also annealed piano-steel.

testing, *repeated* heating and cooling, combined with oscillations for a great number of hours and long rest, having had the effect of rendering the iron much less susceptible to alteration of torsional rigidity from change of temperature.

At the present stage of the inquiry it is impossible to arrive at any definite conclusion as to any relationship between the viscosity of metals and their specific electrical resistance. It would seem indeed that in the case of the pure metals, those which have the greatest viscosity, such as lead and tin, are those whose specific electrical resistance is comparatively great, but even with the pure metals so many circumstances influence the loss of energy that a much more extended investigation must be made ere one can write with sufficient certainty on the point. In the case of the alloys, German silver, platinum-silver, and brass, the values of the logarithmic decrements do not seem to be greater than those pertaining to their components, whereas as regards specific electrical resistance we know that this is not so. Again we encounter the curious fact that whereas with iron the *electrical resistance* is more *increased* by rise of temperature than is the case with any other metal, the *logarithmic decrement* is on the contrary *decreased* by the same cause.

A review of the whole experiments shows that the loss of energy due to *internal* friction of a torsionally vibrating wire does not accord with laws of fluid friction, but with those of *external* friction, inasmuch as the loss of energy from internal friction, like that from external friction, is to a great extent *independent of the velocity*. Whether with external as with internal friction the loss of energy would be independent of the pressure *provided the molecules of the two surfaces were brought into very close proximity*, remains yet to be decided.

V. "Professor Malet's Classes of Invariants identified with Sir James Cockle's Criticoids." By the Rev. ROBERT HARLEY, F.R.S. Received December 10, 1884. Read December 18.

1. Professor Malet, in a paper entitled "On a Class of Invariants," printed in the "Philosophical Transactions" for 1882, Part III, pp. 751–776, says he has not seen it noticed by any mathematician that "in the theory of Linear Differential Equations there are two important classes of functions of the coefficients which have remarkable analogies to the invariants of Algebraic Binary Quantics." He then proceeds to determine the forms of such functions, and to give examples of their application. Soon after the publication of the abstract of Professor Malet's paper in the Proceedings of the Society (vol. 33,

p. 215) I ventured to write to him, calling his attention to the theory of criticoids, and suggesting that it might be convenient to compare his results with those obtained by Sir James Cockle, to whom the theory of criticoids is due.

2. In a footnote to his paper, which I have only lately seen, Professor Malet says that having consulted the memoirs to which I referred him, he thinks "little similarity will be found between Sir James Cockle's results" and his own. The object of this communication is to show that there is not only similarity, but absolute identity, the two classes of functions considered by Professor Malet coinciding in every point with the ordinary and differential criticoids discussed by Sir James Cockle. The researches of the latter mathematician on this subject have appeared at intervals of time in papers scattered and often fragmentary, in different journals during the last twenty years and more. I propose here to reproduce and present in a connected form the principal portions of those researches; and in order to facilitate the comparison of results, I will generally adopt the notation employed by Professor Malet.

3. In a paper, the first of a series,* bearing date 24th December, 1861, Sir James Cockle shows that to every form of

$$\frac{d^2y}{dx^2}+2P_1\frac{dy}{dx}+P_2y=0,$$

where P_1, P_2 are functions of x only, there is a cognate form deducible by the following method. Let $f(x)$ or, more simply, f, be any function of x, and substitute fY for y in the given equation, then

$$\frac{d^2Y}{dx^2}+2Q_1\frac{dY}{dx}+Q_2Y=0,$$

$$\text{where}\quad Q_1=\frac{1}{f}\left(\frac{df}{dx}+P_1f\right),$$

$$\text{and}\quad Q_2=\frac{1}{f}\left(\frac{d^2f}{dx^2}+2P_1\frac{df}{dx}+P_2f\right);$$

$$\text{so that}\quad \frac{d^2f}{dx^2}+2P_1\frac{df}{dx}+(P_2-Q_2)f-\frac{d}{dx}\left\{\frac{df}{dx}+(P_1-Q_1)f\right\}=0;$$

or, developing and reducing by the substitution of $(Q_1-P_1)f$ for $\frac{df}{dx}$, we have

* "On Linear Differential Equations of the Second Order," "The Oxford, Cambridge, and Dublin Messenger of Mathematics," vol. i, pp. 118–124, 164–173, 241–247. See also "On the Integration of Differential Equations," "Mathematical Reprint of the Educational Times," 1868, vol. ix, pp. 105–112.

$$Q_2-Q_1^2-\frac{dQ_1}{dx}=P_2-P_1^2-\frac{dP_1}{dx},$$

which is Professor Malet's function H.

4. In the third paper of the same series it is noticed that the "characteristic" H remains unchanged after the substitution of fY for y; and that if the given linear differential equation be soluble, then

$$\frac{d^2Y}{dx^2}+2Q_1\frac{dY}{dx}+\left(\frac{dQ_1}{dx}+Q_1^2-H\right)Y=0,$$

and therefore also

$$\frac{d^2Y}{dx^2}+2(P_1+a)\frac{dY}{dx}+(P_2+2Pa+a^2)Y=0$$

are soluble, the last two equations being connected by the relation

$$Q_1=P_1+a.$$

5. But these and other results are included in the more general forms subsequently deduced from the linear differential equation of the nth order. It is shown that the form of the function H is independent of the order of the differential equations.

6. "There are critical functions of the coefficients of differential equations analogous to those 'critical functions' of the theory of algebraic equations which, in the theory of quantics, are termed leading coefficients of covariants, peninvariants, or seminvariants."* By a critical function of the roots or coefficients of an algebraic equation is meant a function which remains unaltered when each of the roots is increased or diminished by any quantity whatever, or which remains unaltered when $x+h$ is substituted for x in a given equation in x; it is in fact what Professor Cayley calls a seminvariant, that is, a function which is reduced to zero by one only of the operators which reduce to zero an invariant. The forms of these functions are now well known. About forty years ago Sir James Cockle called attention to them in the pages of the "Mathematician."† Since then he has discussed them more fully in other journals,‡ pointing out the

* Cockle. "Correlations of Analysis," "Philosophical Magazine" for 1862, vol. 24, ser. iv, p. 532, § 2.

† Cockle. "On the Transformation of Algebraic Equations," "Mathematician," vol. i, No. 2, March, 1844, pp. 82–84 (see also vol. i, p. 299); vol. iii, No. 4, November, 1848, pp. 176–178; and Supplement, No. 7, September, 1850, pp. 27–34.

‡ Cockle. "On the Existence of Finite Algebraic Solutions of the General Equations of the Fifth, Sixth, and Higher Degrees," "Philosophical Magazine," vol. 28, ser. iii, March, 1846, pp. 190, 191. "On Critical and Spencian Functions, with Remarks upon Spence's Theory," "Quarterly Journal of Mathematics," vol. iv, pp. 97–111. "On the General Forms of Critical Functions," *ibid.*, pp. 265–270.

important part they play in the theory of algebraic equations; and Mr. James Warren has also, in two papers,* given many illustrations of their properties.

7. In the article "Correlations of Analysis," cited above (see footnote*, p. 47), the following is the method employed for determining the critical differential functions analogous to the critical functions of algebra:—Let fY be substituted for y in the linear differential equation

$$\left(1, P_1, P_2, \ldots P_n \between \frac{d}{dx}, 1\right)^n y=0,$$

and let the transformed equation be

$$\left(1, Q_1, Q_2, \ldots Q_n \between \frac{d}{dx}, 1\right)^n Y=0;$$

then we have, by Leibnitz's theorem,

$$Q_m f=\left(1, P_1, P_2, \ldots P_m \between \frac{d}{dx}, 1\right)^m f,$$

and therefore, making $m=1, 2, 3$, &c., successively, developing, and denoting differentiations by accents,

$$Q_1 f=f'+P_1 f,$$
$$Q_2 f=f''+2P_1 f'+P_2 f,$$
$$Q_3 f=f'''+3P_1 f''+3P_2 f'+P_3 f, \text{ \&c.}$$

The elimination of f, f', f'', &c., by the process employed in dealing with differential equations of the second order, gives

$$Q_2-Q_1^2-Q_1'=P_2-P_1^2-P_1' \quad \ldots\ldots\ldots \quad (\text{H})$$

$$2Q_1^3-3Q_1Q_2+Q_3-Q_1''=2P_1^3-3P_1P_2+P_3-P_1'' \quad \ldots \quad (\text{G})$$

$$-6Q_1^4+12Q_1^2Q_2-4Q_1Q_3-3Q_2^2+Q_4-Q_1'''$$
$$=-6P_1^4+12P_1^2P_2-4P_1P_3-3P_2^2+P_4-P_1''' \quad \ldots \quad (\text{I})$$

which are identical with Professor Malet's functions H, G, I respectively. The method by which Professor Malet derives these functions does not seem to me to differ in any essential particular from the method followed by Sir James Cockle, as indicated above. In order to determine the function I, the process is here carried a step further than it is carried by its inventor in his earlier investigations; but it is proper to mention that the explicit form of that function is after-

* Warren. "Illustrations of the Theory of Critical Functions," "Quarterly Journal of Mathematics," vol. vi, pp. 231–237. "Illustrations of the Theory of Seminvariants or Critical Functions," Part ii, *ibid.*, pp. 372–382.

wards given in a paper on Criticoids,* where other results obtained by Professor Malet are anticipated. For example, it is shown that when, by the usual process, the differential equation

$$\left(1, P_1, P_2, \ldots P_n \between \frac{d}{dx}, 1\right)^n y=0$$

is deprived of its second term, it takes the form

$$\left(1_1, 0, H_1, G, I+3H^2, \ldots \between \frac{d}{dx}, 1\right)^n Y=0;$$

and this is identical with Professor Malet's equation (2).

8. In the same paper the author defines what he means by criticoids, and indicates the different methods by which these functions may be calculated. He says, "A criticoid stands in the same relation to a factorially transformed linear differential equation that a critical function fulfils with respect to a linearly transformed algebraical equation." The word quantoid is used to signify "the sinister of a linear differential equation whereof the dexter is zero." H is called a quadricriticoid, G a cubicriticoid, and I a quarticriticoid of the general quantoid, the degrees of the criticoids being the greatest suffices which occur in them respectively. These criticoids are called basic; they are so called on account of their simplicity. "In each only one single differential coefficient occurs, and into each the coefficient of greatest suffix and the differential coefficient enter only linearly, neither being multiplied into the other or into any differential or other non-numerical coefficient." There are no quadricriticoids which are not functions or multiples of H, and no cubicriticoids which are not functions or multiples of G; but all functions of

$$I+\lambda H^2,$$

where λ is arbitrary, are quarticriticoids.

9. In concluding an inquiry into the analogies between algebraical and differential critical functions, Sir James Cockle anticipates Professor Malet's proof of the general theorem on which the doctrine of criticoids rests, showing that as the transformation of a quantic by a linear substitution leaves the critical functions unaltered, so the transformation of a quantoid by a factorial substitution leaves the criticoids unaltered. (Compare Cockle on "Criticoids," Article 10, with Malet on a "Class of Invariants," p. 752.) There are three ways of calculating ordinary criticoids. First, by elimination; secondly, by depriving the quantic of its second term; thirdly, by means of hyperdistributives,† a process which it is not necessary to explain here, but

* Cockle. "On Criticoids," "Philosophical Magazine" for March, 1870, vol. 39, ser. iv, pp. 201–211.

† Cockle. "On Hyperdistributives," "Philosophical Magazine" for 1872, vol. 43, ser. iv, pp. 300–305.

which will be found to possess distinct advantages over the other two; it is more direct, perfectly general, and easy of application; moreover, it determines the criticoids in their simplest, that is, basic forms.

10. Besides the class of functions hitherto considered, there is another class of a criticoidal nature, the discovery of which is also due to Sir James Cockle. Treating the term Critical as generic, we have at least two species, namely, the algebraical critical species, now commonly known as seminvariants, and the differential critical species, or criticoids, of which there are at least two varieties, namely, the ordinary, and that which Sir James Cockle calls the differential criticoid. But in fact both varieties are differential criticoids, the true distinction between them being that one set of functions remains unaltered when the quantoid is transformed by factorial substitution, and the other when the transformation is effected by change of the independent variable. With the latter, Professor Malet's second class of invariants may be readily identified.

11. Consider the equation of the third order

$$\left(1, P_1, P_2, P_3 \between \frac{d}{dx}, 1\right)^3 y=0,$$

and suppose that, by a change of the independent variable, this is transformed into

$$\left(1, Q_1, Q_2, Q_3 \between \frac{d}{dt}, 1\right)^3 y=0;$$

then, denoting differentiations with respect to x by acute accents, and with respect to t by grave accents, and remembering that $t'=\frac{1}{x^{\grave{}}}$, we have, by the usual formulæ,

$$Q_1=P_1x^{\grave{}}-\frac{x^{\grave{}\grave{}}}{x},$$

$$Q_2=P_2(x^{\grave{}})^2-P_1x^{\grave{}\grave{}}+\left(\frac{x^{\grave{}\grave{}}}{x^{\grave{}}}\right)^2-\frac{1}{3}\cdot\frac{x^{\grave{}\grave{}\grave{}}}{x},$$

$$Q_3=P_3(x)^3;$$

and therefore, since $P^{\grave{}}=P'x^{\grave{}}$,

$$Q_3^{\grave{}}=P_3'(x^{\grave{}})^4+3P_3(x^{\grave{}})^2x^{\grave{}\grave{}}=\frac{P_3'}{P_3}\cdot Q_3x^{\grave{}}+3Q_3(P_1x^{\grave{}}-Q_1);$$

or, what is the same thing,

$$\frac{Q_3^{\grave{}}}{Q_3}+3Q_1=\left(\frac{P_3'}{P_3}+3P_1\right)x^{\grave{}}.$$

Similarly, it is shown that

$$Q_1^{\grave{}}+2Q_1^2-3Q_2=(P_1'+2P_1^2-3P_2)(x^{\grave{}})^2.$$

These relations are given by Sir James Cockle,* and since $x' = \left(\frac{Q_3}{P_3}\right)^{\frac{1}{3}}$, they are obviously the same as Professor Malet's forms I_1 and I_2, viz.:—

$$\frac{Q_3' + 3Q_1Q_3}{Q_3^{\frac{4}{3}}} = \frac{P_3' + 3P_1P_3}{P_3^{\frac{4}{3}}} \quad \ldots\ldots \quad (I_1)$$

$$\frac{Q_1 + 2Q_1^2 - 3Q_2}{Q_3^{\frac{2}{3}}} = \frac{P_1' + 2P_1^2 - 3P_2}{P_3^{\frac{2}{3}}} \quad \ldots\ldots \quad (I_2)$$

see Malet, p. 768.

12. These forms are not, as in the case of the first class of criticoids, independent of the order of the quantoids. This appears from some general results obtained by Sir James Cockle† by the following process. Let t and x be connected by the relation

$$x = \int \frac{dt}{T},$$

where T is a function of t; then $\left(\frac{d}{dx}\right)^n = \left(T\frac{d}{dt}\right)^n$, which gives, by symbolical development,

$$\frac{1}{T^n}\left(\frac{d}{dx}\right)^n = \left(\frac{d}{dt}\right)^n + \frac{n \,.\, n-1}{2} \cdot \frac{T'}{T} \cdot \left(\frac{d}{dt}\right)^{n-1}$$
$$+ \frac{n \,.\, n-1 \,.\, n-2}{2 \,.\, 3}\left(\frac{T''}{T} + \frac{3n-5}{4} \cdot \frac{T'^2}{T^2}\right)\left(\frac{d}{dt}\right)^{n-2}$$
$$+ \&c.,$$

where differentiations with respect to t are denoted, as before, by grave accents. This formula enables us to change the independent variable in any given quantoid in $\frac{d}{dx}$, from x to t. Thus the general linear differential equation

$$\left(1, P_1, P_2, \ldots P_n \between \frac{d}{dx}, 1\right)^n y = 0$$

may be transformed into

$$\left(1, Q_1, Q_2, \ldots Q_n \between \frac{d}{dt}, 1\right)^n y = 0,$$

* Cockle. "On Linear Differential Equations of the Third Order," "Quarterly Journal of Mathematics," vol. vii, pp. 316–326, more particularly equations (l) and (o) on p. 318. This paper bears date 15th August, 1864. See also in the same Journal, vol. viii, pp. 373–383, and vol. xiv, pp. 340–353.

† "On a Differential Criticoid," "Philosophical Magazine" for December, 1875, vol. 50, ser. iv, pp. 440–446.

where

$$Q_1=\frac{P_1}{T}+\frac{n-1}{2}\cdot\frac{T'}{T},$$

$$Q_2=\frac{P_2}{T^2}+(n-2)\left(P_1\cdot\frac{T'}{T^2}+\frac{3n-5}{3\,.\,4}\cdot\frac{T'^2}{T^2}+\frac{1}{3}\cdot\frac{T''}{T}\right).$$

Now $$Q_1^2=\frac{P_1^2}{T^2}+(n-1)\cdot\frac{P_1T'}{T^2}+\left(\frac{n-1}{2}\right)^2\cdot\frac{T'^2}{T^2},$$

and since $P_1'=\frac{P_1'}{T}$,

$$Q_1'=\frac{P_1'}{T^2}-\frac{P_1T'}{T^2}+\frac{n-1}{2}\left(\frac{T''}{T}-\frac{T'^2}{T^2}\right).$$

Hence, λ and μ being indeterminates,

$$\begin{aligned}Q_1'+\lambda Q_1^2+\mu Q_2=&(P_1'+\lambda P_1^2+\mu P_2)\cdot\frac{1}{T^2}\\&+\frac{n-2}{3}\cdot\left(\mu+\frac{3}{2}\cdot\frac{n-1}{n-2}\right)\cdot\frac{T''}{T^2}\\&+\left\{\left(\frac{n-1}{2}\right)^2\lambda+\frac{(n-2)(3n-5)}{3\,.\,4}\mu-\frac{n-1}{2}\right\}\cdot\frac{T'^2}{T^2};\end{aligned}$$

which, making $\mu=-\frac{3}{2}\cdot\frac{n-1}{n-2}$, becomes

$$\begin{aligned}Q_1'+\lambda Q_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}Q_2=&\left(P_1'+\lambda P_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}P_2\right)\cdot\frac{1}{T^2}\\&+(n-1)\left(\lambda-\frac{1}{2}\cdot\frac{3n-1}{n-1}\right)\left(\frac{P_1T'}{T^2}+\frac{n-1}{4}\cdot\frac{T'^2}{T^2}\right);\end{aligned}$$

and therefore, if we assume $\lambda=\frac{1}{2}\cdot\frac{3n-1}{n-1}$, we have

$$\begin{aligned}Q_1'+\frac{1}{2}\cdot\frac{3n-1}{n-1}Q_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}Q_2&\\=\left(P_1'+\frac{1}{2}\cdot\frac{3n-1}{n-1}P_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}P_2\right)\frac{1}{T^2}.&\end{aligned}$$

Finally, observing that $Q_n=\frac{P_n}{T^n}$, we may write,

$$\frac{Q_1'+\frac{1}{2}\cdot\frac{3n-1}{n-1}Q_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}Q_2}{Q_n^{\frac{2}{n}}}=\frac{P_1'+\frac{1}{2}\cdot\frac{3n-1}{n-1}P_1^2-\frac{3}{2}\cdot\frac{n-1}{n-2}P_2}{P_n^{\frac{2}{n}}},$$

a formula which holds for all values of n except 1 and 2. If we make

$n=3$, the formula gives Professor Malet's function I_2 already noticed. See Article 11, equation (I_2). Making $n=4$, we have

$$\frac{12Q_1'+22Q_1^2-27Q_2}{Q_4^{\frac{1}{2}}}=\frac{12P_1'+22P_1^2-27P_2}{P_4^{\frac{1}{2}}} \quad . \quad . \quad . \quad (J_1)$$

Compare Malet on p. 775, where J_1 is written in the form

$$\frac{54P_2-44P_1^2-24\dfrac{dP}{dx}}{\sqrt{P_4}}.$$

Other criticoids of the second class may be derived from the general quantoid by the foregoing process. I give two examples.

13. First. Since $Q_n=\dfrac{P_n}{T^n}$, we have

$$Q_n'=\frac{P_n'}{T^n}-\frac{nP_nT'}{T^{n+1}}=\frac{1}{T^{n+1}}(P_n'-nP_nT'),$$

and therefore

$$Q_n'+\lambda Q_1Q_n=\frac{1}{T^{n+1}}\left\{P_n'+\lambda P_1P_n+\frac{n-1}{2}\left(\lambda-\frac{2n}{n-1}\right)P_nT'\right\},$$

which, on making $\lambda=\dfrac{2n}{n-1}$, becomes

$$Q_n'+\frac{2n}{n-1}Q_1Q_n=\frac{1}{T^{n+1}}\left\{P_n'+\frac{2n}{n-1}P_1P_n\right\},$$

or, as it may be written,

$$\frac{(n-1)Q_n'+2nQ_1Q_n}{Q_n^{\frac{n+1}{n}}}=\frac{(n-1)P_n'+2nP_1P_n}{P_n^{\frac{n+1}{n}}},$$

a formula which holds for all values of n. When $n=2$, the formula gives Professor Malet's function J, viz.,

$$\frac{Q_2'+4Q_1Q_2}{Q_2^{\frac{3}{2}}}=\frac{P_2'+4P_1P_2}{P_2^{\frac{3}{2}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (J)$$

See Malet, pp. 763, 764. When $n=3$, it gives the function I_1, the form of which is exhibited in equation (I_1) of Article 11; and when $n=4$, it gives Professor Malet's function J_3, viz.:—

$$\frac{3Q_4'+8Q_1Q_4}{Q_4^{\frac{5}{4}}}=\frac{3P_1'+8P_1P_4}{P_4^{\frac{5}{4}}} \quad . \quad . \quad . \quad . \quad . \quad (J_3)$$

See Malet, p. 775.

14. Secondly. Carrying the development of $\left(T\dfrac{d}{dt}\right)^n$ sufficiently far to determine the coefficient of $\left(\dfrac{d}{dt}\right)^{n-3}$, we find

$$\frac{1}{T^n}\left(\frac{d}{dx}\right)^n=\left(\frac{d}{dt}\right)^n+\frac{n.(n-1)}{2}\cdot\frac{T'}{T}\cdot\left(\frac{d}{dt}\right)^{n-1}$$

$$+\frac{n(n-1)(n-2)}{2.3}\left\{\frac{T''}{T}+\frac{3n-5}{4}\cdot\frac{T'^2}{T^2}\right\}\cdot\left(\frac{d}{dt}\right)^{n-2}$$

$$+\frac{n(n-1)(n-2)(n-3)}{2.3.4}\left\{\frac{T'''}{T}+2(n-2)\frac{T'T''}{T^2}+\frac{(n-2)(n-3)}{2}\cdot\frac{T'^3}{T^3}\right\}\left(\frac{d}{dt}\right)^{n-3}$$

$+$ &c.

Now applying this formula in the same way that the less developed form in Article 12 was applied, we obtain the following relations:—

$$Q_1=\frac{P_1}{T}+\frac{n-1}{2}\cdot\frac{T'}{T},$$

$$Q_2=\frac{P_2}{T^2}+(n-2)\frac{P_1T'}{T^2}+\frac{n-2}{3}\left\{\frac{T''}{T}+\frac{3n-5}{4}\cdot\frac{T'^2}{T^2}\right\},$$

$$Q_3=\frac{P_3}{T^3}+\frac{3(n-3)}{2}\cdot\frac{P_2T'}{T^3}+(n-3)P_1\left\{\frac{T''}{T^2}+\frac{3n-8}{4}\cdot\frac{T'^2}{T^3}\right\}$$

$$+\frac{n-3}{4}\left\{\frac{T'''}{T}+2(n-2)\frac{T'T''}{T^2}+\frac{(n-2)(n-3)}{2}\cdot\frac{T'^3}{T^3}\right\}.$$

Hence

$$Q_1''=\frac{P_1''}{T^3}-\frac{3P_1'T'}{T^3}-P_1\left\{\frac{T''}{T^2}-\frac{2T'^2}{T^3}\right\}$$

$$+\frac{n-1}{2}\left\{\frac{T'''}{T}-\frac{3T'T''}{T^2}+\frac{2T'^3}{T^3}\right\},$$

$$Q_1Q_1'=\frac{P_1P_1'}{T^3}+\frac{n-1}{2}\cdot\frac{P_1'T'}{T^3}-\frac{P_1^2T'}{T^3}+\frac{n-1}{2}P_1\left\{\frac{T''}{T^2}-\frac{2T'^2}{T^3}\right\}$$

$$+\left(\frac{n-1}{2}\right)^2\left\{\frac{T'T''}{T^2}-\frac{T'^3}{T^3}\right\},$$

$$Q_1Q_2=\frac{P_1P_2}{T^3}+\frac{n-1}{2}\cdot\frac{P_2T'}{T^3}+(n-2)\frac{P_1^2T'}{T^3}$$

$$+\frac{n-2}{3}P_1\left\{\frac{T''}{T^2}+\frac{9n-11}{4}\cdot\frac{T'^2}{T^3}\right\}$$

$$+\frac{(n-1)(n-2)}{2.3}\left\{\frac{T'T''}{T^2}+\frac{3n-5}{4}\cdot\frac{T'^3}{T^3}\right\}.$$

$$Q_1^3=\frac{P_1^3}{T^3}+\frac{3(n-1)}{2}\cdot\frac{P_1^2T'}{T^3}+3\left(\frac{n-1}{2}\right)^2\cdot\frac{P_1T'^2}{T^3}+\left(\frac{n-1}{2}\right)^3\frac{T'^3}{T^3},$$

Employing the indeterminate multipliers μ_1, μ_2, &c., we have

$$Q_1'' + \mu_1 Q_1 Q_1' + \mu_2 Q_3 + \mu_3 Q_1 Q_2 + \mu_4 Q_1^3$$

$$= \frac{1}{T^3}\{P_1'' + \mu_1 P_1 P_1' + \mu_2 P_3 + \mu_3 P_1 P_2 + \mu_4 P_1^3\}$$

$$+ M_1 \,.\, \frac{P_1' T'}{T^3} + M_2 \,.\, \frac{P_2 T'}{T^3} + M_3 \,.\, \frac{P_1^2 T'}{T^3} + M_4 \,.\, \frac{P_1 T''}{T^2}$$

$$+ M_5 \,.\, \frac{P_1 T'^2}{T^3} + M_6 \,.\, \frac{T'''}{T} + M_7 \,.\, \frac{T' T''}{T^2} + M_8 \,.\, \frac{T'^3}{T^3},$$

where

$$M_1 = \frac{n-1}{2}\left\{\mu_1 - \frac{6}{n-1}\right\},$$

$$M_2 = \frac{n-1}{2}\left\{\mu_3 + \frac{3(n-3)}{n-1}\mu_2\right\},$$

$$M_3 = \frac{3(n-1)}{2}\mu_4 + (n-2)u_3 - \mu_1,$$

$$M_4 = \frac{n-2}{3}\mu_3 + (n-3)\mu_2 + \frac{n-1}{2}\mu_1 - 1,$$

$$M_5 = 3\left(\frac{n-1}{2}\right)\mu_4 + \frac{(n-2)(9n-11)}{3\,.\,4}\mu_3 + \frac{(n-3)(3n-8)}{4}\mu_2 - (n-1)\mu_1 + 2,$$

$$M_6 = \frac{n-3}{4}\left\{\mu_2 + \frac{2(n-1)}{n-3}\right\},$$

$$M_7 = \frac{(n-1)(n-2)}{2\,.\,3}\mu_3 + \frac{(n-2)(n-3)}{2}\mu_2 + \left(\frac{n-1}{2}\right)^2\mu_1 - 3\left(\frac{n-1}{2}\right),$$

$$M_8 = \left(\frac{n-1}{2}\right)^3\mu_4 + \frac{(n-1)(n-2)(3n-5)}{2\,.\,3\,.\,4}\mu_3 + \frac{n-2}{2}\left(\frac{n-3}{2}\right)\mu_2 - \left(\frac{n-1}{2}\right)^2\mu_1$$
$$+ (n-1).$$

The conditions of the simultaneous evanescence of M_1, M_2, and M_6 are

$$\mu_1 = \frac{6}{n-1},\ \mu_2 = -\frac{2(n-1)}{n-3},\ \mu_3 = 6,$$

which give $M_4 = 0$, $M_7 = 0$, and

$$M_3 = 3\left(\frac{n-1}{2}\right)\left\{\mu_4 + \frac{4(n-2)}{n-1} - \left(\frac{2}{n-1}\right)^2\right\},$$

$$M_5 = 3\left(\frac{n-1}{2}\right)^2\left\{\mu_4 + \frac{4(n-2)}{n-1} - \left(\frac{2}{n-1}\right)^2\right\} = \frac{n-1}{2}M_3,$$

$$M_8 = \left(\frac{n-1}{2}\right)^3\left\{\mu_4 + \frac{4(n-2)}{n-1} - \left(\frac{2}{n-1}\right)^2\right\} = \frac{1}{3}\left(\frac{n-1}{2}\right)^2 M_3;$$

and therefore, making $\mu_4 = -\left(\frac{2}{n-1}\right)^2\{(n-1)(n-2)-1\}$, we have

$$Q_1'' + \frac{6}{n-1}Q_1Q_1' - \frac{2(n-1)}{n-3}Q_3 + 6Q_1Q_2 - \left(\frac{2}{n-1}\right)^2\{(n-1)(n-2)-1\}Q_1^3$$

$$= \frac{1}{T^3}\Big[P_1'' + \frac{6}{n-1}P_1P_1' - \frac{2(n-1)}{n-3}P_3 + 6P_1P_2$$

$$- \left(\frac{2}{n-1}\right)^2\{(n-1)(n-2)-1\}P_1^3\Big],$$

or, since $T = \left(\frac{P_n}{Q_n}\right)^{\frac{1}{n}}$, we may write

$$\frac{Q_1'' + \frac{6}{n-1}Q_1Q_1' - \frac{2(n-1)}{n-3}Q_3 + 6Q_1Q_2 - \left(\frac{2}{n-1}\right)^2\{(n-1)(n-2)-1\}Q_1^3}{Q_n^{\frac{3}{n}}}$$

$$= \frac{P_1'' + \frac{6}{n-1}P_1P_1' - \frac{2(n-1)}{n-3}P_3 + 6P_1P_2 - \left(\frac{2}{n-1}\right)^2\{(n-1)(n-2)-1\}P_1^3}{P_n^{\frac{3}{n}}}$$

From this form, which holds for all values of n greater than 3, we can readily deduce Professor Malet's function J_2; for, making $n=4$, we get

$$\frac{Q_1'' + 2Q_1Q_1' - 6Q_3 + 6Q_1Q_2 - \frac{20}{9}Q_1^3}{Q_4^{\frac{3}{4}}} = \frac{P_1'' + 2P_1P_1' - 6P_3 + 6P_1P_2 - \frac{20}{9}P_1^3}{P_4^{\frac{3}{4}}} \quad . \quad . \quad . \quad . \quad (J_2)$$

Professor Malet (see p. 775) writes J_2 in the form

$$\frac{40P_1^3 - 36P_1\frac{dP_1}{dx} - 18\frac{d^2P_1}{dx^2} - 108P_1P_2 + 108P_3}{P_4^{\frac{3}{4}}},$$

which differs from the dexter of (J_2) by a numerical factor only.

15. From the foregoing inquiry it appears that the first class of functions H, G, I are given explicitly in the memoirs to which I referred Professor Malet; and that the second class I_1, I_2, J_1, may be derived from general forms contained in those memoirs by simply assigning particular numerical values to n. Further, it is shown in the last two articles that by an obvious extension of Sir James Cockle's method we may obtain forms which include the functions J, J_2, J_3 as particular cases. Professor Malet's Classes of Invariants are thus completely identified with Sir James Cockle's Criticoids.

16. Professor Malet calls these functions invariants, but they would seem to partake more of the nature of seminvariants or critical functions. If we agree to call an expression critical when it remains

unchanged after a change of one variable, and ultra-critical when it so remains after a change of more than one, then seminvariants and invariants are respectively critical and ultra-critical algebraical functions. Criticoids may be called ordinary, or of the first class, when the dependent variable is changed, and extraordinary, or of the second class, when the independent variable is changed. A criticoid, whether of the first or second class, is in the nature of a seminvariant, not of an invariant; it does not correspond with an invariant. It is critical, not ultra-critical. In differential expressions we have critical functions or criticoids. But we have not in general ultra-critical functions, or, as it is proposed to call them, Invaroids. To obtain such functions it is necessary to change two variables, the dependent and the independent. In a recent letter to me Sir James Cockle suggests that in a limited number of cases it may be possible by means of semicritical relations* to form invaroids, that is, ultra-critical functions of the calculus analogous to the invariants or ultra-critical functions of algebra. Such functions may have a place in the general theory, but it is not necessary to pursue the subject here.

17. In tracing out the analogies between algebraical and differential equations, Sir James Cockle has been led to consider another class of criticoidal functions, namely, those connected with partial differential equations.† How the criticoidal functions of the first and second class may be combined, what are the uses of criticoids, and what is the part they play in the theory of the solution of differential equations,‡ are points illustrated by numerous examples in the memoirs from which I have drawn most of the materials of this communication.

The Society adjourned over the Christmas Recess to Thursday, January 8th, 1885.

* Let $f(Q)$ be a function of the Q's and their differential coefficients with respect to t, and let $f(P)$ be the same function of the P's and their differential coefficients with respect to x; also let $\phi(X)$ denote a function of $X\left(=\left(\dfrac{dt}{dx}\right)^{-1}\right)$ with respect to x. Then if

$$f(Q)\,dt^m - f(P)\,dx^m = \phi(X)\,dx^m \quad \ldots \quad (A),$$

he calls (A) a semicritical relation. Thus

$$\left(Q_2 - Q_1^2 - \frac{dQ_1}{dt}\right)dt^2 - \left(P_2 - P_1^2 - \frac{dP_1}{dx}\right)dx^2 = \frac{n+1}{3}\cdot\frac{1}{X^{\frac{1}{2}}}\cdot\frac{d^2X^{\frac{1}{2}}}{dx^2}\cdot dx^2,$$

is a semicritical relation which, notation apart, includes (11) of Art. 53 of the concluding paper "On Linear Differential Equations of the Third Order," cited in footnote *, p. 51. In like manner (17) of Art. 55 of that paper is a semicritical relation.

† Cockle. "On Fractional Criticoids," "Philosophical Magazine" for May, 1871, vol. xli, ser. 4, pp. 360–368.

‡ In the concluding paper "On Linear Differential Equations of the Third

January 8, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Dr. Morrison Watson was admitted into the Society.

In accordance with the announcement made from the Chair at the last Meeting, the question of Mr. James Bateman's re-admission was put to the vote. It was decided in the affirmative, whereupon the Treasurer declared that Mr. Bateman was re-admitted into the Society.

The following Papers were read :—

I. "Experimental Researches in Magnetism." By Professor J. A. EWING, B.Sc., F.R.S.E., University College, Dundee. Communicated by Sir WILLIAM THOMSON, F.R.S. Received December 5, 1884.

(Abstract.)

The paper describes in detail experiments of which preliminary notices have already been published in the "Proceedings of the Royal Society," vol. 34, p. 39, and in the "Philosophical Magazine," November, 1883. The experiments relate to—

(1.) The magnetic susceptibility of iron and steel, the form of the magnetisation curve, and the changes of magnetism caused by cyclic changes of magnetising force.

(2.) The influence of vibration on magnetic susceptibility and retentiveness.

(3.) The influence of permanent strain on magnetic susceptibility and retentiveness.

Order," Arts. 53–58, it is shown that the general form of which the criticoid J_2 is a particular case, enables us to solve for all orders the problem of destroying the second and third terms, that is, it makes it depend on the solution of equations of the orders 1 and 2. For other applications of the theory, see Cockle "On Primary Forms," Arts. 6–14, "Philosophical Magazine" for February, 1875, vol. xlix, ser. 4, pp. 134–142. See also the "Mathematical Reprint from the Educational Times," vol. 34, pp. 109, 110; Question 6503.

(4.) The energy expended in producing cyclic changes of magnetisation.

(5.) The ratio of residual to total induced magnetism.

(6.) The changes of induced and residual magnetism caused by changes of stress.

(7.) The effects of constant stress on magnetic susceptibility and retentiveness.

(8.) The changes of magnetism caused by changes of temperature.

(9.) The effect of temperature on magnetic susceptibility.

The experiments were conducted on pieces of metal which gave as near an approach to the condition of uniform magnetisation as is practically attainable. In some cases rings were used in a manner similar to that employed by Stoletow and Rowland, except that in the present case the process of magnetisation was effected in a series of sudden steps, the total magnetisation being found by summing up the inductive effect of the steps. In most cases, however, the author used straight pieces of wire, whose length was 300 or 400 times their diameter. These permitted the use of a direct magnetometric method of observation, and consequently the magnetising force could be applied and removed continuously and slowly, instead of by sudden steps. Preliminary experiments had satisfied the author that wires of this length gave as near an approach to uniform magnetisation as could be practically obtained by using rings.

Curves are given which show the behaviour of iron and steel in various states of temper, when subjected to a first application of magnetising force, and also to subsequent cyclic changes of magnetising force, such as complete or partial removal and re-application, or reversal. The curves are drawn by plotting either $\mathfrak{I}$, the intensity of magnetisation, or $\mathfrak{B}$, the magnetic induction, in relation to $\mathfrak{H}$, the magnetising force: the characteristics of these curves and their relation to the physical state of the piece under examination are pointed out. Curves so drawn invariably exhibit the static lagging action to which the author (in a former paper) gave the name "hysteresis," any cyclic change of $\mathfrak{H}$ giving rise to a more or less nearly closed *loop* in the curve. Attention was previously drawn to these loops by Warburg, who also anticipated the author in pointing out their important physical meaning, namely, that the area of a loop, or $-\int \mathfrak{I} d\mathfrak{H}$, is the measure of the energy expended (per unit of volume) in performing the cycle of magnetisation which the loop describes. In the present paper numerous absolute measurements of this energy are given, especially of the energy which is thus dissipated in each reversal of the magnetism of a piece of iron or steel. These show that while the dissipation of energy by reversal of magnetism is very much smaller in soft iron than in hard iron or steel, even in the latter its amount is very trifling, so that the principal part of the heat which is produced in the cores of

electromagnets must be due chiefly to other causes than this static hysteresis, and is, in fact, due almost wholly to the induction of so-called Foucault currents in the cores. The relation of this hysteresis to Weber's theory of molecular magnets, as extended by Maxwell, is discussed, and the insufficiency of Maxwell's extension noticed.

By vibrating a piece of soft iron during the application and removal of magnetising force, the effects of hysteresis are almost entirely removed, and the iron is then found to possess almost no retentiveness. But when the application and removal of magnetising force are effected without mechanical disturbance, the retentiveness of soft iron is found to be even greater than that of steel. In some cases ·93 per cent. of the whole induced magnetism of a piece of annealed iron was found to remain, on the complete removal of the magnetising force. It is pointed out that there is no discrepancy between this result and the well-known fact that a *short* iron core of an electro-magnet retains almost no magnetism when the current in the magnet is interrupted. In that case the ends of the magnet itself, after the interruption of the current, exert a sufficient reversed magnetising force to destroy almost entirely the residual magnetism. But when tested under the conditions which give uniform magnetisation and avoid the demagnetising influence of the ends, soft annealed iron is more retentive than even the hardest steel.

Examples are given showing that the influence of permanent set in the curve of magnetisation is so marked as to give a criterion by which a strained piece may be readily distinguished from an annealed piece of metal, and that strain diminishes very greatly the magnetic retentiveness of iron.

Numerical values of the coefficients of permeability (μ) and of susceptibility (κ) are given for a number of samples of iron and steel, and the relation of these coefficients to $\mathfrak{B}$ and $\mathfrak{I}$ is exhibited graphically after the manner of Rowland. The greatest value of μ refers to soft annealed iron while under mechanical vibration, and is about 20,000.

The next part of the paper deals with the effects of stress (consisting of longitudinal pull) on the magnetic susceptibility and retentiveness of iron, and the subject is taken up in two different ways. In one the magnetic effects are observed when a load hanging from the wire under examination is gradually applied, removed, and otherwise varied, the wire being then either kept in a magnetising field of constant value, or freed from all magnetising force. In the other method the load is kept constant, and magnetising force is applied, removed, and otherwise varied. The two methods give results which would be identical were it not for (1) hysteresis in the relation of magnetism to load, in a constant field, and (2) hysteresis in the relation of magnetism to field, under constant load. The relation of mag-

netism to varying load in a constant field is examined at great length, both in soft annealed wire, and in wire hardened by stretching, and the effects, which are very complex, are exhibited graphically. They also afford an easy criterion by which a stretched piece can be distinguished from an annealed piece, and, both in stretched and annealed metal, they exhibit static hysteresis, which can be more or less completely wiped out by mechanical vibration. The connexion of the author's present work with the earlier results of Villari, and the extensive researches of Sir W. Thomson, is pointed out.

Numerous curves are given, showing the relation to $\mathfrak{I}$ to $\mathfrak{H}$ when the process of magnetisation is conducted while the piece under test is maintained in a constant state of stress. These show very clearly the position of what Thomson has called the "Villari critical point," or point at which the sign of the effect of stress on magnetism becomes reversed; and it is shown that this reversal is associated rather with a particular value of magnetisation than with a particular value of magnetising force, the same reversal being found to occur when the magnetism dealt with is wholly residual.

Instances are given of very curious molecular reminiscences of previous loads, which were found to exist even when the previous loading had been performed at a time when the metal was entirely destitute of magnetism, and although the loads whose effects were found to remain were much less than loads previously applied to the same wire, and were consequently unable to produce mechanical change of any ordinary kind. These residual effects of former loads became visible on the next magnetisation of the piece. It is shown that these effects of previous loads can be almost obliterated by subjecting the wire to vibration before beginning to magnetise it.

The ratio of residual to induced magnetism in soft and hardened iron and steel has been examined for various degrees of magnetisation, with the result of showing that the ratio passes a maximum when the magnetisation is moderately strong, and then falls off as the state of so-called saturation is reached. The influence of the presence of stress on this ratio and on residual magnetism generally has also been investigated.

The last part of the paper deals more briefly with the effect of temperature on magnetism, a subject already largely treated by G. Wiedemann and others. The present experiments were specially directed to discover whether there is hysteresis in the relation of magnetism to temperature when the temperature suffers cyclic changes. Careful experiments on both induced and residual magnetism have led the author to conclude that within the range of temperature dealt with by him, there is no perceptible hysteresis in the relation of magnetism to temperature. Curves are also given exhibiting the process of magnetising the same piece at different temperatures. These cross each

other in the same manner as the corresponding curves for different values of stress, a fact to be anticipated from the earlier discoveries of Baur.

The experiments, which have been of a very extended character, were made, during 1881–1883, in the Laboratory of the University of Tokio, Japan, with the help of Japanese students, Messrs. Fujisawa, Tanakadate, Tanaka, and Sakai, to whom the author is indebted for much valuable assistance. The results have been almost without exception, reduced to absolute measure, and are for the most part presented graphically in curves which accompany the paper.

II. "On certain Definite Integrals. No. 12." By W. H. L. Russell, F.R.S. Received December 8, 1884.

The following theorem must be implicitly known, although I have never seen it in print:—

$$\text{Let } \xi=\lambda x+\mu y+\nu z, \qquad \Delta x=\Lambda\xi+\mathrm{M}\eta+\mathrm{N}\zeta.$$
$$\eta=\lambda' x+\mu' y+\nu' z, \qquad \Delta y=\Lambda'\xi+\mathrm{M}'\eta+\mathrm{N}'\zeta.$$
$$\zeta=\lambda'' x+\mu'' y+\nu'' z, \qquad \Delta z=\Lambda''\xi+\mathrm{M}''\eta+\mathrm{N}''\zeta.$$

Then $\iiint dxdydz\phi(\lambda x+\mu y+\nu z,\ \lambda' x+\mu' y+\nu' z,\ \lambda'' x+\mu'' y+\nu'' z)$
$$=\iiint \Theta\phi(\xi,\eta,\zeta)d\xi d\eta d\zeta,$$

where the limits of the first integral, and consequently those of the second, are given by an equation of limits, and Θ is a well-known constant. Now let

$$\mathrm{P}=ax^3+by^3+cz^3+a_1x^2y+a'_1x^2z+b_1y^2x+b'_1y^2z+c_1z^2x+c'_1z^2y+mxyz,$$

and let the expression break up into three linear factors, so that

$$\mathrm{P}=(\lambda x+\mu y+\nu z)(\lambda' x+\mu' y+\nu' z)(\lambda'' x+\mu'' y+\nu'' z),$$

which will subject the constants a, b, a_1, &c., to three conditions.

Then we have $\displaystyle\iiint\frac{dxdydz\,x^{l-1}y^{m-1}z^{n-1}}{\sqrt[r]{\mathrm{P}}}=$

$$\Sigma\frac{\rho\Delta^3}{(\Lambda+\Lambda'+\Lambda'')(\mathrm{M}+\mathrm{M}'+\mathrm{M}'')(\mathrm{N}+\mathrm{N}'+\mathrm{N}'')}\,\frac{\Gamma\left(\alpha-\frac{1}{r}\right)\Gamma\left(\beta-\frac{1}{r}\right)\Gamma\left(\gamma-\frac{1}{r}\right)}{\Gamma\left(1+\alpha+\beta+\gamma-\frac{3}{r}\right)},$$

where l, m, n are positive integers, ρ a function of $\lambda, \mu, \nu, \lambda'$, &c., which is different for each term, and $\alpha+\beta+\gamma=l+m+n$. The limits of the integral are given by the equation $x+y+z=1$.

Let P be the sum of the three cubes

$$(\lambda x+\mu y+\nu z)^3+(\lambda' x+\mu' y+\nu' z)^3+(\lambda'' x+\mu'' y+\nu'' z)^3,$$

which implies one condition between the constants a, b, and c. Then we have

$$\iiint dxdydzx^{l-1}y^{m-1}z^{n-1}\phi(\mathrm{P})=\Sigma\rho\frac{\Gamma\frac{\alpha}{3}\Gamma\frac{\beta}{3}\Gamma\frac{\gamma}{3}}{\Gamma\left(\frac{\alpha}{3}+\frac{\beta}{3}+\frac{\gamma}{3}\right)}\int_0^1 du\phi(u)u^{\frac{\alpha}{3}+\frac{\beta}{3}+\frac{\gamma}{3}-1}.$$

where other things being the same, the limits are determined by the equation

$$\mathrm{P}=1.$$

It is scarcely necessary to point out that these investigations may be much extended when the radical sign is different, when the algebraical function under it is of a higher degree, and when there are more than three variables.

I enter now on a different subject.

In a partial differential equation

$$\int \mathrm{F}\left(x\frac{d}{dx},\ y\frac{d}{dy}\right)u=0,$$

substitute for (u) a series of which the general term is $\mathrm{A}x^m y^n$. Then if this series satisfies the equation, term by term, we have an algebraical equation $f(m,\ n)=0$, whence $m=\phi(n)$, and the equation is satisfied by an infinite series of the form

$$\mathrm{A}x^{\phi(n)}y^n+\mathrm{B}x^{\phi(n_1)}y^{n_1}+\mathrm{C}x^{\phi(n_2)}y^{n_2}+\ \ldots$$

Now suppose $y^{\phi(n)}$ to be expressed in the form $\int \mathrm{P}\mathrm{Q}^n d\phi$, then the series is transformed into

$$\mathrm{A}\int \mathrm{P}(x\mathrm{Q})^n d\phi+\mathrm{B}\int \mathrm{P}(x\mathrm{Q})^{n_1}d\phi+\ \ldots$$

where A, B, &c., are arbitrary constants.

Since the number of these constants is infinite, we may write this (after Poisson)

$$\int \mathrm{PF}(x\mathrm{Q})d\phi$$

As an example, take the equation

$$\alpha.\frac{d^{\mu+1}u}{d\eta^\mu d\xi}+\beta\frac{d^\mu u}{d\eta^{\mu-1}d\xi}+\gamma\frac{d^{\mu-1}u}{d\eta^{\mu-2}ud\xi}+\ \ldots=a\frac{d^\nu u}{d\eta^\nu}+b\frac{d^{\nu-1}u}{d\eta^{\nu-1}}+c\frac{d^{\nu-2}u}{d\eta^{\nu-2}}+\ \ldots$$

This may be written

$$\alpha\left(y\frac{d}{dy}\right)^{\mu}\left(x\frac{d}{dx}\right)u+\beta\left(y\frac{d}{dy}\right)^{\mu-1}\left(x\frac{d}{dx}\right)u+\gamma\left(y\frac{d}{dy}\right)^{\mu-2}\left(x\frac{d}{dx}\right)u+\ldots$$

$$=a\left(y\frac{d}{dy}\right)^{\nu}u+b\left(y\frac{d}{dy}\right)^{\nu-1}u+c\left(y\frac{d}{dy}\right)^{\nu-2}u+\ldots$$

Hence if Ax^my^n be a specimen term, we have, substituting for u,

$$(an^{\mu}+\beta n^{\mu-1}+\gamma n^{\mu-2}+\ldots)m=an^{\nu}+bn^{\nu-1}+cn^{\nu-2}+\ldots,$$

whence we have

$$m=\frac{an^{\nu}+bn^{\nu-1}+cn^{\nu-2}+\ldots}{\alpha n^{\mu}+\beta n^{\mu-1}+\gamma n^{\mu-2}+\ldots}.$$

Hence we have to reduce

$$\epsilon^{\log_\epsilon x}\cdot\frac{an^{\nu}+bn^{\nu-1}+cn^{\nu-2}+\ldots}{\alpha n^{\mu}+\beta n^{\mu-1}+cn^{\mu-2}+\ldots}$$

to the form $\int PQ^n d\theta$. This is easily done, for the function is equivalent to

$$\epsilon^{\frac{\log_\epsilon x}{r_1+s_1n}+\frac{\log_\epsilon x}{r_2+s_2n}+\frac{\log_\epsilon x}{r_3+s_3n}+\ldots}$$

Now we have

$$\epsilon^{\frac{\log_\epsilon x}{r_1+s_1n}}=\frac{\sqrt{r_1+s_1n}}{2\sqrt{\pi\log_\epsilon x}}\int_{-\infty}^{\infty}\epsilon^{v-\frac{(r_1+s_1n)v^2}{4\log_\epsilon x}}dv,$$

and
$$\sqrt{r_1+s_1n}=\frac{2}{\sqrt{\pi}}(r_1+s_1n)\int_0^{\infty}\epsilon^{-(r_1+s_1n)v^2}dv.$$

Hence we see that the function can be reduced to the form

$$(A+Bn+Cn^2+\ldots+En^{\mu})\iint PQ^n d\theta d\phi\,.\iint P_1Q_1{}^n d\theta_1 d\phi_1\,.$$

Now if
$$Fx=Mx^{\alpha}+Nx^{\beta}+Px^{\gamma}+\ldots$$

$$\left(x\frac{d}{dx}\right)^r Fx=M\alpha^r x^{\alpha}+N\beta^r x^{\beta}+\ldots$$

and consequently we have, if we put

$$\left(x\frac{d}{dx}\right)^r F(u)=F_r(x),$$

$$u=A\iint\ldots PP_1\ldots F(QQ_1\ldots)d\theta d\phi\ldots$$
$$+B\iint\ldots PP_1\ldots F_1(QQ_1\ldots)d\theta d\phi\ldots$$

It will be seen at once that this is an extension of Poisson's solution of the equation $\frac{du}{dt}=a^2\frac{d^2u}{dx^2}$. There is only one arbitrary function in my solution, and only one in Poisson's, as thus treated. But he has given one with two arbitrary functions, and I believe a similar investigation would apply to my general equations if the equation,

$$\frac{an^{\nu}+bn^{\nu-1}+\ \ldots}{\alpha n^{\mu}+\beta n^{\mu-1}+\ \ldots}=m,$$

were solved with regard to (n), and thus n found in terms of (m).

III. "The Force Function in Crystals." By ALFRED EINHORN, Ph.D. Communicated by G. MATTHEY, F.R.S. Received November 27, 1884.

(Abstract.)

The first part of the paper which appears at present restricts itself to the consideration of the Tesseral, Tetragonal, and Rhombic systems. By means of a well founded assumption in regard to the stress-distribution in crystals of the above systems, the equilibrium conditions are deduced which further involve that the boundary of the configuration must either be plane or spherical.

It also appears that the statical conditions of the agency which causes crystallisation are the same as those so well investigated for gravitation and electricity.

The paper is divided into three chapters. The first chapter treats of the "Foundation of the Assumption." The assumption is that the stress upon any particle can only be transmitted in six direction-lines respectively at right angles in pairs to the three crystallographic axes—it is a consequence of the internal structure which is shown to be analogous to that of an ordinary cannon-ball pile by means of the cleavage properties, the external form and inertia relations of crystals.

The second chapter—"Derivation of the Force Function"—applies the three general differential equilibrium equations of an elastic solid subject to internal forces to the stated stress-distribution. In order to effect this it was necessary to deduce some peculiarities of the force function in a system of uniform density in equilibrium, and subject to internal forces when referred to the three principal axes of inertia through the mass centre. The character of the attracting agency here becomes evident.

The third heading, "Determination of the Boundary." Under this

heading the nature of the boundary is determined, and is shown to be either plane or spherical. And by the application of Green's theorem it also becomes clear that inasmuch as the statical conditions of the crystallising agent are now understood, the force functions derived in the preceding chapter can be independently deduced without aid of the assumption from any one of the primitive forms of the systems under consideration.

IV. "On some Applications of Dynamical Principles to Physical Phenomena." By J. J. THOMSON, M.A., F.R.S., Fellow of Trinity College, and Cavendish Professor of Physics in the University of Cambridge. Received December 16, 1884.

(Abstract.)

In this paper an attempt is made to apply dynamical principles to study some of the phenomena in electricity, magnetism, heat, and elasticity. The matter (including, if necessary, the ether) which takes part in any phenomenon is looked upon as forming a material system, and the motion of this system is investigated by means of general dynamical methods, Lagrange's equations being the method most frequently used. To apply this method, it is necessary to have coordinates which can fix the configuration of the system, so in the first part of the paper coordinates are introduced which fix the configuration of the system, so far as the phenomena we are considering are concerned, *i.e.*, we introduce coordinates which can fix the geometrical, the electrical, the magnetic, the heat, and the strain configuration of the system; we call these, coordinates of the x, y, z, u, and w types respectively. Some of these coordinates only enter the expression for the kinetic energy through their differential coefficients, and may be called gyroscopic coordinates, as such coordinates are of frequent occurrence in problems about gyroscopes.

The terms which involve the x, y, z, u, and w coordinates in the expression for the kinetic energy will be of fifteen different types.

There will be those terms which are quadratic functions of the differential coefficients of the x coordinates, and corresponding terms for the y, z, u, and w coordinates; so that there are in this set terms of five different types, all of which may exist in actual dynamical systems. There are ten types of terms involving the products of differential coefficients of two coordinates of different kinds. These types are considered in order, and it is shown that we have experimental evidence for the existence of only two of them, viz.,

those which involve the product of the differential coefficients of coordinates of the x and w and of the y and z types. Thus we may write those terms which depend on the coordinates x, y, z, u, w, in the expression for the kinetic energy of any real dynamical system, in the form

$$\begin{aligned} &\tfrac{1}{2}\{xx\}\dot{x}^2+\ \dots\\ &+\tfrac{1}{2}\{yy\}\dot{y}^2+\ \dots\\ &+\tfrac{1}{2}\{zz\}\dot{z}^2+\ \dots\\ &+\tfrac{1}{2}\{uu\}\dot{u}^2+\ \dots\\ &+\ \{xw\}\dot{x}\dot{w}+\dots\\ &+\ \{yz\}\dot{y}\dot{z}+\ \dots \end{aligned}$$

when the term $\{xx\}\dot{x}^2+\ \dots$ indicates a quadratic function of the differential coefficients of the coordinates of the x type.

Each of these terms is separately considered, and the physical phenomena to which it corresponds are deduced. The method used may be illustrated by considering a term of the form

$$\{\lambda\mu\}\dot{\lambda}\dot{\mu},$$

when λ and μ may be any of the five types of coordinates which we are considering.

Then, if T be the kinetic energy, we have by Lagrange's equations

$$\frac{d}{dt}\frac{dT}{d\dot{\lambda}}-\frac{dT}{d\lambda}=\text{external force of the type } \lambda.$$

If, instead of T, we consider the term $\{\lambda\mu\}\dot{\lambda}\dot{\mu}$, we see by this equation, and the corresponding equation for the coordinate μ, that if this term exist there will be a force tending to increase λ equal to

$$-\left\{\frac{d}{dt}((\lambda\mu)\dot{\mu})-\frac{d}{d\lambda}(\lambda\mu)\dot{\lambda}\dot{\mu}\right\}$$

and one tending to increase μ, equal to

$$-\left\{\frac{d}{dt}((\lambda\mu)\dot{\lambda})-\frac{d}{d\mu}(\lambda\mu)\dot{\lambda}\dot{\mu}\right\}$$

and if $(\lambda\mu)$ be a function of any other coordinate v there will be a force tending to increase v equal to

$$\frac{d}{dv}(\lambda\mu)\dot{\lambda}\dot{\mu}.$$

Thus, to take an example, Wiedemann has discovered that a longitudinally magnetised iron wire becomes twisted when an electric current flows through it. If we call $\dot{y}$ the current through the wire, $z\dot{w}$ the intensity of magnetisation, and a the twist round the axis

of the wire, then Wiedemann's discovery shown that there must be a term in the kinetic energy equal to

$$f(a)\lambda z\dot{w}.$$

where $f(a)$ denotes some function of a

Thus there will be a force of the type y, *i.e.*, an electromotive force, along the wire equal to

$$-\frac{d}{dt}\{f(a)z\dot{w}\},$$

so that, if we twist a magnetised iron wire, an electric current will flow along the wire, which will last as long as the wire is being twisted: this is known to be the case. Again, there will be a force of the type z—*i.e.*, a magnetising force along the wire equal to

$$f(a)\dot{y},$$

so that when a current flows along a twisted wire it magnetises it; this effect has also been observed. Thus, from the original experiment, we have been able, by the use of Lagrange's equation, to deduce two other phenomena. It is shown in the paper that the method indicates a great many relations between various physical phenomena. Some of these have been observed, but there are several which seem not to have been investigated; as an example of the latter, it is proved that from the effect observed by Villari and Sir William Thomson, namely, that when the intensity of magnetisation is below a certain value, an increase in the strain of a magnetised soft iron is accompanied by an increase in the magnetisation, it follows that when the magnetising force is small, a soft iron bar will contract instead of expanding on magnetisation.

Lagrange's equations were applied with great success by Maxwell to obtain the equations of the electromagnetic field.

It is also shown that the effects due to the potential energy of a system A can be produced by the kinetic energy of a system B connected with A, if the configuration of B is such that it can be fixed by gyroscopic coordinates. And thus we may look on the potential energy of any system (A) as being the kinetic energy of a gyroscopic system (B) connected with A, and so regard all energy as kinetic. If we do this it will simplify some of the dynamical principles very much. We may take the principle of Varying Action as an example: if all the energy is kinetic, its magnitude will remain constant by the Conservation of Energy, and then the principle of Least Action takes the very simple form that, with a given quantity of energy, any material system will, by its unguided motion, pass from one configuration to another in the least possible time, where, in the

phrase "material system," we include the gyroscopic systems whose motions produce the same effect as the potential energy of the original system, and two configurations are not supposed to coincide unless the configurations of these systems coincide also.

V. "On a New Constituent of the Blood and its Physiological Import." By L. C. WOOLDRIDGE, D.Sc., M.B., Demonstrator of Physiology in Guy's Hospital. Communicated by Professor M. FOSTER, Sec. R.S. Received December 16, 1884.

In a paper on the Origin of the Fibrin Ferment, published in "Proc. Roy. Soc.," vol. 36, I showed that there exists, dissolved in the plasma, a body which can give rise to fibrin ferment.

I have proceeded with my investigations, and have succeeded in making some additions to our knowledge of this subject, which I here describe. As my researches are not complete, I confine myself to as brief an account as possible.

The subject is best studied in the blood of peptonised dogs. But as I showed in the above quoted paper, similar results are obtained from normal salt plasma, so that the results are not peculiar to pepton blood. The body, the presence of which gives rise to fibrin ferment, can be isolated from pepton plasma in the following very simple manner:—The plasma having been completely freed from all corpuscular elements by means of the centrifuge, is cooled down to about 0°. The plasma, which was previously perfectly clear, becomes rapidly turbid, and after standing for some time in the cool, a very decided flocculent precipitate forms. I have already described this observation in a short note, "Ueber einen neuen Stoff des Blut-Plasmas," in du Bois Reymond's "Archiv für Physiologie," but it is necessary for me to allude to it here.

Now it is this body which gives rise to the fibrin ferment. So long as the former is present in considerable quantity the latter clots readily on passing through it a stream of carbonic acid, or on dilution, and at the same time a very considerable quantity of fibrin ferment makes its appearance.

By prolonged cooling the greater part of this substance can be removed, and with its gradual removal the plasma clots less and less readily with CO_2, and less and less ferment is formed, till finally it becomes practically incoagulable, *i.e.*, forms only a faint trace of fibrin after several days. If some of the substance be again added to the plasma, it regains its power of clotting with CO_2.

(The substance must be added before it has stood very long: see under.)

It must be understood that the plasma, previous to the passage of the CO_2, is quite free from fibrin ferment, so that there can be no question of the ferment being mechanically removed by the precipitate.

Moreover, that it is really the body removable by cold which gives rise to the fibrin ferment, and not any second body which is mechanically carried down with the former, is shown by the fact that the diffusion of a large quantity of inert finely-divided precipitate through the plasma, and its subsequent removal by the centrifuge, does not in any way do away with the power of the plasma to clot.

It is therefore justifiable to assume that when pepton plasma clots readily and completely with CO_2, it must contain this new body in some quantity, and that when it will not clot, or only very imperfectly, after *repeated* treatment with CO_2 or dilution, this new body must be present in very small quantity.

Now I have found that the behaviour of pepton plasma with CO_2 varies very considerably with the diet on which the animal is fed, and whether the animal is fasting or has recently been fed. In some cases it clots readily, in others practically not at all.

Out of eight dogs fed on very lean meat only one gave a plasma which clotted at all fully, and in this case the clotting went on for two days. From all the others the plasma, in spite of repeated treatment with CO_2, only gave rise after two or three days to a scarcely perceptible fibrin membrane. The animals were killed about eighteen hours after the last meal.

Of six dogs fed on fat and meat for several days all gave a plasma which clotted rapidly and fully in from twenty minutes to one hour after the CO_2 treatment. The animals were killed about eighteen hours after being fed.

Of two dogs fed on bread and meat both gave a readily coagulable plasma.

One day's feeding on fat does not produce any effect; that is the blood of a dog thus fed behaves like that of a dog fed on a lean meat.

A dog fed for some days on fat and meat, was for five days previous to being killed put on fat alone; as a consequence it practically starved, as it ate scarcely anything. The blood from this dog clotted very incompletely.

Simple starvation for three days did away with the influence of fat in another case.

These results only hold good for dogs in health. In a dog with a suppurating wound, kindly placed at my disposal by Mr. Horsley, the plasma, in spite of a lean meat diet, clotted with very great rapidity, and contained an enormous quantity of the new body. All the other dogs were healthy, but were badly nourished when they came into my hands.

It is necessary for these experiments that the peptonisation should be complete.

For the complete understanding of these results, I must return to a further consideration of this new constituent of the plasma.

The turbidity which appears on first cooling the plasma, if examined microscopically, is found to consist of a great number of minute pale transparent bodies of a rounded shape, much resembling small organised bodies, such for instance as the stroma of the red corpuscles, except that they are of very various size, but generally much smaller than red corpuscles. They have a great tendency to run together into granular masses.

At first the precipitate is soluble on re-warming the plasma slightly, but it soon undergoes change, and loses the power of redissolving by heat. If the substance be collected by means of the centrifuge it forms a disk or thin membrane at the bottom of the tube, much reminding one of fibrin, but closer examination shows that it presents marked differences from the latter, and that, in truth, it much more closely resembles the peculiar viscid body obtained by destroying leucocytes with dilute alkalies, &c.

On longer standing, however, it becomes in most cases still further changed, and is then undistinguishable from ordinary fibrin, swelling in dilute HCl like the latter. For further details as to the properties of this substance, I refer to my paper quoted above.

We have already seen that this substance gives rise to fibrin ferment, but it does more than this in inducing coagulation.

Pepton plasma is not coagulable with fibrin ferment. If we take some plasma rich in this new substance, and by means of CO_2 induce coagulation, we obtain, on removing the clot, a serum which has the power of inducing exceedingly rapid coagulation in a new portion of plasma, and this, when the serum has regained its alkalinity. This serum contains ferment, but inasmuch as ferment is not sufficient to induce coagulation, it must also contain some other substance. Now leucocytes have exactly the same power. They give rise to ferment, but they also give rise to the other substance necessary for coagulation.

We see, therefore, that we have dissolved in the plasma a body exerting the same influence on the induction of coagulation as the leucocytes.

I think this is the strongest chemical proof that can be brought that the leucocytes break down to make, at any rate, a part of the proteid constituents of the plasma, and have shown above the influence which diet, &c., has on the extent of this process, a fact of obvious interest for the question of assimilation.

There is, however, another important conclusion to be drawn from these observations, viz., that one must admit, in addition to the

ordinary fermentative fibrin formation, that fibrin may be deposited from blood by simple physical means, without any ferment process; for this new substance becomes, as I have stated, true fibrin, and yet the plasma does not contain ferment. Possibly this mode of fibrin formation is of importance in the formation of a thrombus.

The peculiar microscopical characters should also be noted, as possibly affording an explanation of the observations made by Osler, Bizzozero, Hayem, and others. I refer of course to the granules, Blutplättchen, hæmatoblasts, described by these authors.

As I am actively engaged on this subject, and as I hope before long to produce a complete account of my researches on the coagulation of the blood, I have purposely confined myself to the briefest outlines.

VI. "On the Marsupial Ovum, the Mammary Pouch, and the Male Milk Glands of *Echidna hystrix*." By WILHELM HAACKE, Ph.D., Museum Director to the Public Library, Museum, and Art Gallery of South Australia, Adelaide, and late Assistant Director to the Zoological Institute, Jena. Communicated by the President. Received November 12, 1884.

On the 3rd of August, 1884, a number of living specimens of *Echidna hystrix* were brought to Adelaide from Kangaroo Island, where they had been captured some days previously. I was unable to procure more of them than two, a male and a female, as the others had been disposed of before I heard of them. But those two afforded me the good fortune of making a discovery that, in our days, perhaps no naturalist would have expected to make. I found an *egg* in the mammary pouch of the female, and was thus enabled to prove that *Echidna* is really an oviparous mammal.

This discovery was made on the 25th of August; it was announced, and the egg was exhibited at the meeting of the Royal Society of South Australia on the 2nd of September; the scientific society referred to being the first one on record, the members of which had an opportunity of examining an egg laid by one of the Monotremata.

On September 14th I killed and dissected the female *Echidna* that had laid the egg. Neither the pouch nor the oviducts nor the uterus contained any more ova, but all the female organs were well developed. Only the right ovary contained what evidently was a corpus luteum, but there was only one. This corpus luteum deserved its name, as it was orange coloured instead of having a pale fleshy tint as is the case with the ovisacs still containing undeveloped ovula; it was not quite so large and round as the largest of the ovisacs.

The *Echidna egg*, when taken out of its mother's pouch, unfortunately burst, and it was found to be almost wholly decomposed inside. The mother having been worried by being captured and during captivity accounts for the state the egg was found in. As the shell of the egg broke so soon I am unable to give the egg's dimensions as accurately as could be desired; however, I think I am nearly correct in stating its length as 15 m.m., and its width as 13 mm.; it will thus be seen that the egg is nearly globular in shape. In a preliminary notice to the "Zoologischer Anzeiger" the dimensions given are somewhat incorrect.

The shell of the egg was parchment-like, like that of the eggs of many reptiles. Its outer surface was considerably smoother than the inner one. Its thickness was about $\frac{1}{2}$ mm.

The inner surface of the egg-shell was covered with a loose membrane which on microscopical examination showed a network of what I supposed to be blood-vessels; no other trace of an embryo could be discovered.

It will have been observed already that I speak of one "mammary pouch" or "marsupium" only, whereas Professor Owen described as pouches two semilunular fossæ, at the fundi of which he found the orifices of the mammary glands ("Phil. Trans.," vol. 155). But as the mammary pouch serves at one period as an incubator for the "marsupial ovum," it must at least at this time have a shape enabling it to undertake this function. Accordingly, we find in my specimen one deep pouch large enough to hold, although not wholly to conceal, a gentleman's watch, having its fundus in the median plane of the body towards the cloaca, and running out into shallower fossæ towards the mammary areolæ, beyond which towards the anterior end of the body it gradually disappears. The skin forming the pouch is thinner than that of the rest of the ventral integument, and the pouch and its lips are covered more thinly with hair, the latter being shorter, however, and standing thicker on the mammary areolæ, where very conspicuous tufts of hair are easily to be observed. Although the marsupial integument is devoid of subcutaneous muscles, the animal can considerably modify the shape of the pouch, probably by the surrounding integumentary muscles.

Quite recently Professor Gegenbaur ("Morphologischer Jahrbuch," 1884) examined two adult females of *Echidna setosa* for the existence of mammary pouches, but without success. He, therefore, inclines to think that the pouches are only periodically developed organs. Although I have not sufficient evidence to prove this supposition, I believe it is the correct one; how could the statements of Professor Owen, of Professor Gegenbaur, and of myself otherwise be reconciled? I believe the pouch attains its greatest development during the time it serves as incubator for the egg, and it consists of a pair of semi-

annular fossæ during the period the mother suckles her progeny, disappearing afterwards altogether.

The examination of two male *Echidnæ* which I had an opportunity to carry out during September led me to what I believe is another new discovery. Respecting the mammary glands of *Ornithorhynchus* Professor Owen ("Phil. Trans.," vol. 122, 1832) says:—"In the male both Dr. Knox and Professor Meckel have been unable to detect these glands, and after a careful scrutiny with the same view in a well preserved specimen of that sex, I have not succeeded in detecting more than a few minute lobules occupying a space of about four lines in situations corresponding to those in the female, but the nature of which, from the total absence of corresponding foramina on the external surface of the integument, may still be doubted."

Furthermore, in the "Zoological Record" for 1876 I read:—"C. Creighton suggests that the *glandula femoralis*, which opens by a long duct on the spurs of the male in *Ornithorhynchus* and *Echidna*, may prove to be the homologue of the mammary gland of the female." "J. Anat. Phys.," xi, pp. 29–31.

This latter quotation of a certainly original suggestion shows that up to the end of 1876 nothing could have been known about the unmistakable homologue of the mammary gland of the female which I recently discovered in two male *Echidnæ*, and I am not aware that any new facts to elucidate this question have been published up to the present time.

I was, therefore, not a little surprised to find in the two male *Echidnæ* I examined, in situations corresponding to those in the female, similar tufts of short hair quite as plain as those indicative of the mammary areolæ in my female. On the animals being skinned, I found without any difficulty rudimentary mammary glands. In the largest of my two specimens the one mammary gland forms a mass about 8 mm. long by 4 mm. wide, the lobules being about 2 mm. long. The other gland is a little larger. The glands are built after the same plan as the female glands, and contain a considerable number of lobules.

My anatomical preparations and the *Echidna egg* discovered by me will remain in the Adelaide Museum.

In conclusion, I should not forget to mention that I am aware of Mr. W. H. Caldwell's discovery of the oviparity of the Monotremata, made about the same time as it seems at which my discovery was made, and that the British Association at their Montreal meeting received the information of Mr. Caldwell's discovery about the same date at which my *Echidna egg* was exhibited at a meeting of the Royal Society of South Australia. But this is all I know at present about Mr. Caldwell's discovery.

January 15, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On the Chemical Composition of the Cartilage occurring in certain Invertebrate Animals." By W. D. HALLIBURTON, M.D., B.Sc. (Lond.), Sharpey Physiological Scholar, University College, London. Communicated by Professor E. A. SCHÄFER, F.R.S. (from the Physiological Laboratory, University College, London). Received December 24, 1884.

At Professor Lankester's suggestion I have submitted to chemical analysis the cartilages occurring in Sepia and in Limulus.

The basis of the cartilage is a chondrin-like body which gives the reactions of mucin and gelatin; indeed chondrin, as it occurs in the ordinary hyaline cartilage of vertebrates, is now regarded by many as a mechanical mixture of these two bodies. But in the cartilages of the two invertebrates in question the gelatinous element is exceedingly small, and no gelatinisation occurs on the cooling of the hot watery extract.

In addition to this, however, the cartilage of both these animals differs from that of vertebrates in containing a certain small percentage of chitin. In the case of Limulus 1·01 per cent., and of Sepia 1·22 per cent. of chitin in the dry state is present.

I have also demonstrated that chitin exists in the liver of the king crab, though whether in the connective tissue or in the liver cells themselves I cannot say. The connective tissue element is very abundant in the liver of this animal, and it seems probable, looking at the part that chitin plays as a supporting structure in these animals, that it really forms in this instance a partial basis for the connective tissue.

The way in which chitin was demonstrated to exist was the same in all three cases, viz.:—

1. After digesting with potash, a residue insoluble in boiling alkalies remains behind.

2. This residue, which when washed and dried is obtainable in a white amorphous condition, is insoluble in weak acids; but in concentrated mineral acids it is soluble in the cold.

3. On boiling the solution in sulphuric acid, a body which has the power of reducing cupric salts is formed.

4. On boiling the solution in hydrochloric acid it turns brown, and on evaporating this solution to dryness a body crystallises out which has all the properties of hydrochlorate of glycosamine.

I prepared some of this body from the chitin contained in the exoskeleton of cockroaches, and also obtained from Professor Lankester some crystals of the same body which Professor Gamgee had kindly sent him.

I was thus enabled to compare the crystalline body I had obtained from the invertebrate cartilage with that of the pure hydrochlorate of glycosamine, and they were found to agree in the following points:—

a. Crystalline form: rhombic prisms of the monoclinic system; measurement of the angles gave the same result in all cases.
b. Action of polarised light: *nil.*
c. Solubilities: easily soluble in water, soluble with difficulty in alcohol.

These results are especially interesting as showing that chitin is not a body which is exclusively epiblastic in origin, but in these three instances at least occurs in mesoblastic structures.

II. "On the Constant of Electromagnetic Rotation of Light in Bisulphide of Carbon." By Lord RAYLEIGH, F.R.S. Received December 29, 1884.

(Abstract.)

A complete account is here given of the experiments briefly referred to in the Preliminary Note,* and of others on the same plan of more recent date. As regards the method, it may be sufficient to add to what was there said, that the electric currents were estimated by comparing the difference of potential generated by the current in traversing a known resistance with that of a standard Clark cell, the value of the cell being known by converse operations, in which the current was measured by a special electromagnetic apparatus.† Allowance

* "Proc. Roy. Soc.," vol. 37, p. 146, June, 1884.

† "On the Electrochemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells." "Proc. Roy. Soc.," vol. 37, p. 142, June, 1884.

being made for temperature, the determination of the currents by this method was abundantly accurate and very simple.

The results are grouped in three series, of which the first two were considered in the Preliminary Note. In both of them the same tube was used, the principal difference being that in the first the light traversed the tube three times, and in the second but once. In the third series another tube was employed, and some improvements in respect to thermal insulation were introduced. The readings were taken with a double image prism in place of the ordinary analysing nicol, a substitution by which it is believed some advantages were obtained.

From the fifteen sets of observations of Series I, we find as the rotation of sodium light in bisulphide of carbon at 18° corresponding to a difference of potential equal to unity C.G.S. the value ·04203 minute. From the four observations of Series II we get in like manner ·04198 minute, and from the seven observations of Series III ·04202 minute. The last value is adopted as the most probable.

In an appendix some remarks are made upon polarimetry in general, especially in relation to the half-shade method. A device proposed by M. Becquerel for augmenting the precision with which rotations can be determined with the aid of a half-wave plate is considered, and the conclusion is arrived at that no advantage can thus be obtained.

III. "Absorption-spectra Thermograms." By Captain ABNEY, R.E., F.R.S., and Colonel FESTING, R.E. Received December 31, 1884.

When employing a grating for visual work, the choice of a medium which shall absorb the overlapping parts of orders of the spectrum other than that under examination is a comparatively simple affair. When working photographically, however, it becomes necessary to know what invisible rays the medium will cut off. For instance, in investigations in the infra-red of the spectrum, we have found it necessary to ascertain whether media which absorb the blue and allow the red rays to pass would also transmit the above-mentioned parts of the spectrum.

Photography would seem to be the simplest method of experimenting in this direction; but the results not being quantitative, as are those obtained by a thermopile, we found it better to work with that instrument.

It should be remembered that we were dealing with the infra-red part of the spectrum, in which the energy is so great as to be well shown by the thermopile; in the more refrangible part the indications

of the thermopile are so feeble that photography would be more serviceable.

The absorbing media usually used were :—

1. A solution of potassium dichromate.
2. Ruby glass combined with orange glass.
3. Deep orange glass alone (often called "stained red").
4. Iodine dissolved in an aqueous solution of potassium iodide.
5. Iodine in alcohol.
6. Iodine in carbon disulphide (the violet rays being absorbed by canary glass).
7. Dyes of different kinds, combined with coloured glasses.
8. Deep coloured cobalt glass and orange glass.

Of these the 1st and 3rd were employed when the green rays had not to be cut off; the 2nd, 4th, and 5th when the whole of the more refrangible end of the spectrum down to the orange had to be absorbed, while the 6th cut off very nearly the whole of the visible spectrum. The dyes employed were soluble in either alcohol or water.

With No. 8 (deep cobalt and orange glasses) a very long exposure to the dark rays was found necessary to obtain a satisfactory impression on a sensitive plate.

In these experiments we used the thermopile described in our paper in the "Proc. Roy. Soc." (No. 232, 1884). The source of radiation was an incandescence lamp maintained by a Grove's battery of 40 cells, the filament being approximately at the temperature of 1650°.

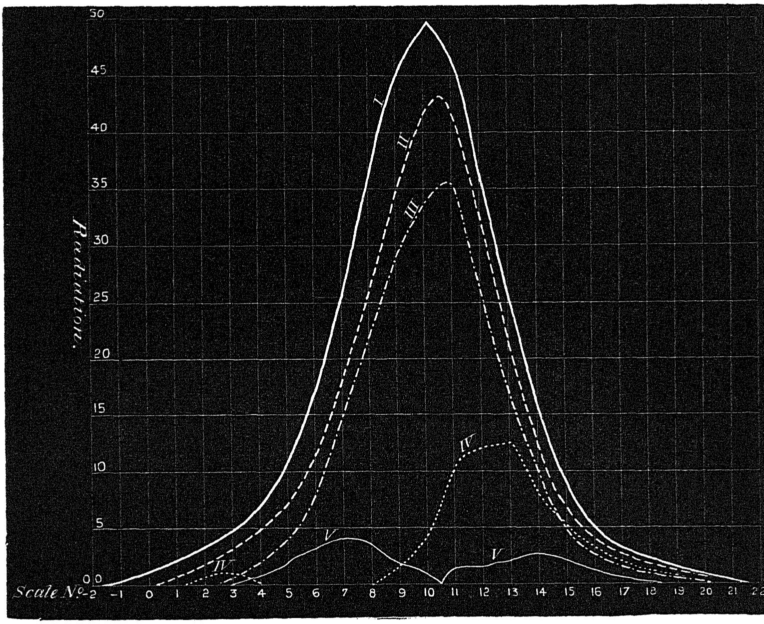
An image of the filament was thrown by a lens on the slit of a collimator, behind which was the dispersing prism. The rays emerging from this were collected by a second lens into a spectrum at a definite distance from the prism, and the thermopile was passed along this spectrum, as described in our paper above mentioned. The absorbing media were placed in front of the slit of the collimator, glass cells one-eighth of an inch of internal thickness being used for containing the liquids.

The thermogram of the radiation unabsorbed, except by the prism and lenses, was first made (see Curve I, figs. I and II). An empty cell was next placed in front of the slit; the thermogram (Curve VI, fig. II) shows that no special, but a general absorption has been produced.* A comparison of Curves II, III, and V with Curve I, in fig. I, shows the absorption of radiant energy by orange glass, ruby glass, and cobalt glass respectively.

Through the kindness of Messrs. Chance Brothers we can state that the orange colour is given by the oxides of silver and antimony combined, whilst the ruby colour is due to cuprous oxide. The

* The empty cell-curve has been corrected for reflection from the two inner surfaces of the glass, so that all comparisons are made under similar conditions.

FIG. 1.



Curve I. Naked incandescence lamp.
Curve II. Absorption of orange glass.
Curve III. " ruby "
Curve IV. " green "
Curve V. " dark cobalt glass.

cobalt colour, as is well known, is given by cobalt. A glance at the curves shows why any combination in which the last is employed would necessitate a prolonged exposure, as it will be seen that it entirely absorbs the infra-red rays about the part of maximum energy. The use of this glass should therefore be avoided, if possible, when the infra-red rays are to be used in photography.

In a paper by Captain Abney, which appeared in the "Philosophical Magazine" (vol. x, 1880), it was shown that reversal of the photographic image was caused by rays of low refrangibility. If a photographic plate which has been exposed to white light in the camera be subsequently exposed to radiations of low refrangibility, as for instance to light passing through a combination of ruby and orange glass, we should expect that there would be a gradual extinction of the effect of the white light. This is true in practice, though it rarely happens that the after exposure to such radiation is sufficiently prolonged to be hurtful. A case, however, might arise when a knowledge—or perhaps it

should rather be said an appreciation—of this fact would be of practical use. In such a case the visible radiation coming through a combination of cobalt and orange glass might be successfully employed.

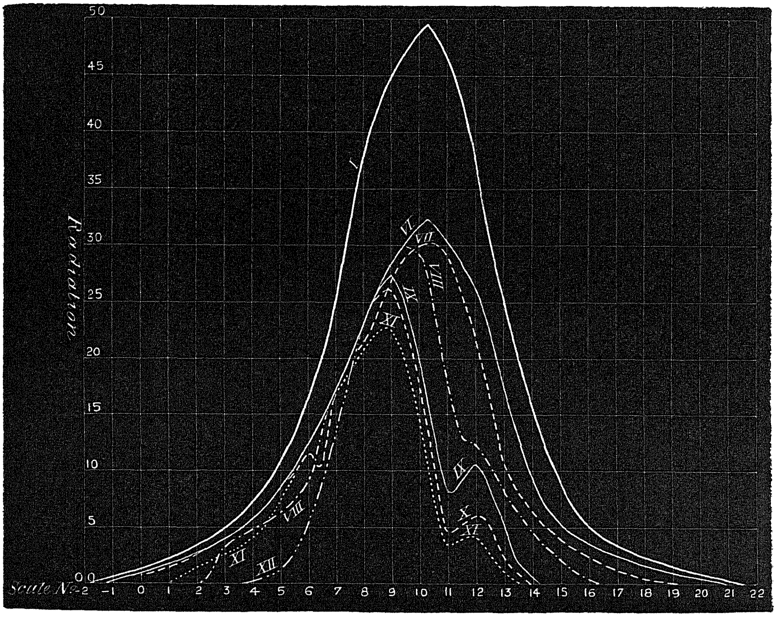
Curve IV is the absorption-spectrum thermogram of a green glass, which obtains its colour from a combination of cupric oxide and iron oxides. An examination of the prismatic spectrum photographed through such glass has led us to suspect that green glass is transparent to certain rays of very low refrangibility. The thermogram fully confirms this. Some four years ago one of us proposed to the Meteorological Committee of the Royal Society the use of green glass for thermometer bulbs, in the belief that it totally absorbed the infra-red rays. This is now found to be a mistake, and some other colouring matter must be sought for for this purpose.

Fig. II illustrates some interesting facts in connexion with absorption. Curves I and VI, as already stated, are the thermograms of the lamp without and with an empty glass cell in front of the slit. Curve VII is the absorption thermogram of benzene. As far as Scale No. 6 there is no appreciable absorption, but from that point there is a drop in the curve, which proves to be coincident with the radical band of the group of benzene compounds, which we mapped photographically (see "Phil. Trans.," 1882). At a point between 7 and 8 on the scale absorption again commences, and although of some small extent, is appreciable as far as the end of the thermogram, thermal action being traceable nearly down to Scale No. 20, or wavelength 27,000. This point we presume to consider of some importance, for we have shown that there is reason to believe the existence of some compound of the aromatic series between us and the sun, as the radiant band is coincident with a very strong line in the solar spectrum. The solar spectrum as determined by the thermopile being coterminous in this direction with that of the incandescence lamp, had it been found that benzene cut off the lower part of the latter, our belief as to the indications of benzene in the solar spectrum would have been much shaken.

Curve VIII is the thermogram of a deep-coloured solution of iodine in alcohol. This terminates near Scale No. 16. We found a slight dip near Scale No. 6; this was too slight to be conveniently shown on the diagram. It is coincident in position with the alcohol radical band. The thermogram of uncoloured alcohol is the same as that of Curve VIII, with the exception that some further thermal action in the visible part of the spectrum has to be registered.

Curve IX is the thermogram of the absorption-spectrum of a saturated solution of common salt in water. Rock salt, as is well known, allows nearly all radiation to pass, and it seemed desirable to ascertain what effect it would have in solution. The effect of the salt appears to have been a mitigation of the absorptive power of the

FIG. 2.



Curve I. Naked incandescence lamp.
Curve VI. Absorption of empty cell.
Curve VII. ,, of cell and $\frac{1}{8}$-inch of benzene.
Curve VIII. ,, ,, $\frac{1}{8}$-inch iodine solution in alcohol.
Curve IX. ,, ,, ,, sodium chloride saturated solution in water.
Curve X. ,, ,, ,, water.
Curve XI. ,, ,, ,, iodine in an aqueous solution of potassium iodide.
Curve XII. ,, ,, ,, iodine in carbon disulphide.

water, as the curve shows the same qualitative absorption as Curve No. 10, which is that due to $\frac{1}{8}$-inch of water, the absorption being equivalent to what would have been produced by a diminished thickness of water. This is a fact of some importance theoretically, and has been confirmed by experiments with other solutions.

Curve XI is that due to the absorption of iodine in an aqueous solution of potassium iodide. Compared with Curve X it shows that the molecular structure of potassium iodide is such that it intensifies the absorption due to water alone, no trace of the spectrum being found beyond Scale No. 14.

Curve XII is the thermogram of the absorption-spectrum of a solu-

Scale Number.	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	Curves.
	Naked light.	Orange glass.	Ruby glass.	Green glass.	Dark cobalt glass.	Empty glass cell.	Benzene.	Iodine in alcohol.	NaCl in water.	Water.	Iodine in potassium iodide and water.	Iodine in carbon disulphide.	Approximate wave-lengths.
—1	0	0	0	0	0	0	0	0	0	0	0	0	5500
0	1	0	0	0	0	0¾	0½	0	0½	0½	0	0	5850
2	3	1½	0	0½	0	2½	2½	0	2½	2½	0½	0	6500
3	..	..	..	1	0	..	..	3	..	2½	..	0	6850
4	7	4½	1¾	0	0¾	5¾	5¾	3	5¾	5¾	4	0½	7250
5	11	7	4	0	2	8	8	6	8	8	7	1½	7750
6	17	11	8	0	3	12½	11½	9	12½	12½	12½	6	8400
6½	..	..	..	0	..	..	11	..	..	..	..	..	8800
7	27	17	15	0	4	17½	17½	17½	17½	17½	17	14	9200
8	37½	25	22	0	3¾	24	22	24	24	24	21	24	10100
9	45	35	29	1½	2	29	28	29	27½	26½	22	29	11250
10	49	42	34	4	1	32	30	29	19	18	13	32	12500
10½	48½	43	35½	..	0	31½	30	..	..	..	..	31½	13200
11	46½	42	35½	10	1½	30½	29	18	8	4½	3½	30½	13900
11½	..	..	..	..	..	..	..	13	9½	5	4	..	14600
12	35½	32	25½	12	1½	26	21	12½	10½	6	4	26	15300
13	25	21	17	12½	2	17	12	8	5	3	1½	17	16800
14	15½	12	10	8	2½	9	6½	4½	0½	0	0	9	18300
15	9	7	5	5½	2¼	5½	4	2	0	0	0	5½	19800
16	4	4	2½	3½	1½	4	2	0½	0	0	0	4	21300
18	2	1¼	1	1¼	0¼	1¼	0½	0	0	0	0	1¼	24500
20	0½	0¼	0	0¼	0	0½	0	0	0	0	0	0¼	27000

tion of iodine in carbon disulphide. It differs from the curve due to the empty cell only in the visible and shortest wave-length portion of the invisible spectrum, as might be expected. We have also examined plain carbon disulphide and carbon tetrachloride, but have found only small traces of absorption with these liquids.

We have found that alum solution gives a curve which differs but little from No. XI in the least refrangible part of the spectrum; the alum seems to intensify the absorptive power of the water in which it is dissolved. It has been so often stated that an alum solution cuts off all rays of low refrangibility (or as it is incorrectly and commonly said, all "heat rays") that we were not prepared for the comparatively small effect that it produces. It may be said that, roughly, one thickness of a saturated solution of alum in water is equivalent to a double thickness of water, and not more. Judging by the thermograms, even this would be an exaggeration of the truth; but the use of glass in the cells, prisms, and lenses diminishes the effect as found when the total radiation is taken directly.

We may add that dyes seem only to absorb in the visible spectrum, and to have but little, if any, action in the invisible regions.

The positions which we assign to the maxima of energy in the different absorption spectra of glasses do not agree with those that have been published; but as ours are the result, not of one set of experiments, but, in some cases, of dozens, we feel fairly confident as to their correctness.

IV. "Observations on the Upper Partial Tones of a Pianoforte String, struck at one-eighth of its Length." By Alfred James Hipkins (of John Broadwood and Sons, London). Communicated by Alexander J. Ellis, F.R.S. Received January 7, 1885.

This is a postscript to my paper on the *harmonics* of such a string, read on the 20th of November, 1884. According to Professor Helmholtz's theories, the tone of a struck string is compounded of a number of *simple partial* tones, with the ratios of their frequencies as 1, 2, 3, 4, &c. The *harmonics* are themselves also *compound* tones of which the primes or lowest partials are the partials of the original tone. These are produced on damping the other partials by touching them at a node. Now, in my former paper, I showed that by so touching I could bring out twenty different harmonics of the string, and among these the 8th and 16th. Young's law, however, makes an harmonic impossible to produce, if its node is the point struck. Hence the string being struck at one-eighth of its length, no 8th or 16th

harmonics should be audible, simply because the 8th partial would thus be damped. But as they were heard in my experiments, it was objected that they were created by the system of stopping with a piece of felt at the exact node, which was the plan that I adopted. I therefore endeavoured to find an independent mode of determining what partials were present without having recourse to "resonators," against which others have raised similar objections, for if the 8th partial did not exist, there could be no 8th harmonic.

No doubt existed respecting the presence of the first four partials, and the chief interest centred about those near to the 8th. On referring to the table in my last paper, it will be seen that, when the piano was tuned in the equal temperament, e'', $b''\flat$, e''' were sharper than the 5th, 7th, and 10th partials would be, and f''' much flatter than the 11th partial, and that g'', c''', d''' were very nearly of the same pitch as the 6th, 8th, and 9th partials. This led me to flatten or sharpen the strings of those notes on the piano to a small extent, leaving the original note untouched, and taking care to damp the unison strings of all the notes with the tuner's "wedges," so that only one string of each note sounded. The following were the results of sounding the original note simultaneously with the others in succession:—

The "beats" of untouched e'' with the bass c of 135·2 vibrations were very rapid, but by flattening the e'' slightly, the beats became slow and very distinct. This unmistakably proved the existence of a simple tone nearly of the pitch of this flattened e'', in the compound tone of c. That is, the beats established the existence of the 5th partial.

Similarly, on very slightly flattening g'', the beats were quite distinct, hence the existence of the 6th partial was established.

The $b''\flat$ had to be rather more flattened, but then the beats came out clearly, establishing the 7th partial. And on producing the 7th harmonic by touching the strings at a node, the beats were recognised as precisely the same.

The c''' was flattened very slightly indeed, and while c was struck loudly, this flattened c''' was struck lightly. The beats were much fainter, but quite distinct and well heard by all present. The result was that the existence of the 8th partial on the c string, struck at one-eighth of its length by a pianoforte hammer, was fully established. The reason of its existence may be the elastic nature of the hammer, which necessarily affected the string on each side of the node. But the important point is that, although the c string, struck at one of the nodes of the 8th partial, was untouched at any other of its nodes (as it was touched in three of those nodes successively in the experiments of my last paper), the 8th partial clearly existed. The beats also came out with the 8th harmonic when produced.

In the same way, the 9th partial was proved to exist, though the beat was still faint. The beats for the 10th partial (flattening e''') were better.

On sharpening f''', the beats of the 11th partial were just sensible. They lasted such an extremely short time that they were recognised with difficulty. It therefore did not seem worth while to try further.

These experiments conclusively proved the existence of the 5th, 6th, 7th, 8th, 9th, 10th, and 11th partials on a pianoforte string struck with a pianoforte hammer at one-eighth of its length, and that the 7th was comparatively powerful, while the 8th and 9th, though faint, were distinct. The node of the 7th was 0·8 inch from the striking place, and that of the 9th was 0·6 inch, so that the 9th was more affected than the 7th, but the 10th was 1·1 inch from the striking place, and hence probably was less affected than the 9th. The curious point is that the 8th partial was most decidedly not destroyed.

These experiments were all witnessed by Mr. A. J. Ellis, and one of Messrs. Broadwood's principal tuners, Mr. Pryer, who altered the pitch of the strings as required.

January 22, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Observations on the Chromatology of Actiniæ." By C. A. MacMunn, M.A., M.D. Communicated by Professor M. Foster, Sec. R.S. Received January 8, 1885.

(Abstract).

In this paper I have given the results of an examination of the following Actiniæ, mainly with regard to the spectroscopy of their colouring matters, viz., *Actinia mesembryanthemum*, *Bunodes crassicornis*, *Bunodes ballii*, *Sagartia bellis*, *Sagartia dianthus*, *Sagartia parasitica*, *Sagartia viduata*, *Sagartia troglodytes*, and *Anthea cereus*.

The previous work of Moseley, Krukenberg, Geddes, the Hertwigs, Brandt, and Heider, is first referred to.

The presence of a colouring matter in *Actinia mesembryanthemum*, which can be changed into alkaline hæmatin, hæmochromogen, and acid and alkaline hæmatoporphyrin is proved by appropriate tests, hence this Actinia contains a pigment nearly related to hæmoglobin. The same colouring matter has been found in *Bunodes crassicornis*, *Sagartia dianthus* (in small amount), *Sagartia viduata* (traces), and a closely connected pigment in *Sagartia troglodytes*, both in ectoderm and endoderm.

Another colouring matter is found which is special to this species in *Sagartia parasitica;* it is capable of existing in the oxidised and reduced state, and is found in the ectoderm, while in the interior of the animal a pigment changeable into hæmochromogen was detected; the spectroscopic characters of the former are described in detail.

In the mesoderm and elsewhere in *Actinia mesembryanthemum* and in *Bunodes crassicornis*, a green pigment occurs which is undistinguishable from biliverdin. It gives the same play of colours (in solution) with nitric acid, and the same changes of spectra which accompany the colour changes with that reagent in the case of biliverdin from the bile of vertebrates. The presence of a chlorophyll-like spectrum has been detected in the tentacles of *Bunodes ballii* and *Sagartia bellis*, as well as in *Anthea cereus* (in which last it had been previously detected by former observers), and this spectrum has been found to belong to the "yellow cells" which are found abundantly in the tentacles and elsewhere in these species. The various solutions of this colouring matter give the same spectra in all three. Professor Lankester's and Mr. Sorby's statement that this spectrum is similar to chlorofucin has been verified by a comparison with a solution of chlorofucin from *Fucus serratus*.

This colouring matter has been shown—contrary to the opinion expressed by Krukenberg—to be quite different from enterochlorophyll, also from plant chlorophyll, and other animal chlorophylls.

In every case the "yellow cells" are proved to have a cellulose wall, and to contain starch.

There are various new facts ascertained which cannot be explained by means of a short abstract.

The conclusions arrived at may in part be summed up as follows:—

(1.) *Actinia mesembryanthemum* contains a colouring matter which can be changed into hæmochromogen and hæmatoporphyrin, this is present in the other species mentioned above, and from its characters it is provisionally named *Actiniohæmatin*.

(2.) It is not actiniochrome (a pigment found by Professor Moseley in the tentacles of *Bunodes crassicornis*), as its band occurs nearer the violet than that of actiniochrome. Moreover, both actiniochrome and actiniohæmatin can be extracted with glycerin, in which the latter is convertible into hæmochromogen, but the former remains unchanged.

Actiniochrome is generally confined to the tentacles, and is not respiratory, actiniohæmatin occurs in the ectoderm and endoderm, and is respiratory.

(3.) A special colouring matter is found in *Sagartia parasitica*, different from either of the above, and this too exists in different states of oxidation. It is not apparently identical with that obtained by Heider from *Cerianthus membranaceus*.

(4.) In the mesoderm and elsewhere in *Actinia mesembryanthemum* and other species, a green pigment occurs which alone and in solution gives all the reactions of biliverdin.

(5.) *Anthea cereus*, *Bunodes ballii*, and *Sagartia bellis*, yield to solvents a colouring matter resembling chlorofucin, and all the colouring matter, which in them shows this spectrum, is derived from the "yellow cells," which are abundantly present in their tentacles and elsewhere. It is not identical with any animal or plant chlorophyll, as is proved by adding reagents to its alcoholic solution.

(6.) When "yellow cells" are present, there appears to be a suppression of those colouring matters which in other species are of respiratory use.

All readings are reduced to wave-lengths, and the spectra described illustrated by means of sixty-five maps of spectra. The "yellow cells" are also drawn alone and stained with iodine in iodide of potassium, and with Schultze's fluid.

II. "On the Origin of the Proteids of the Chyle and the Transference of Food Materials from the Intestine into the Lacteals." By E. A. Schäfer, F.R.S. From the Physiological Laboratory, University College, London. Received January 12, 1885.

In consequence of the discovery that in many of the lower Metazoa the ingestion of food particles is the result of an amœboid activity of individual cells of the organism, and that digestion and assimilation may also occur within the protoplasm of cells thus endowed with amœboid activity, attention has of late been directed to the part which such cells may play in promoting absorption from the alimentary canal of Vertebrates.

It is well known that lymph-cells occur in large numbers in the mucous membrane of the intestine, which is everywhere beset with them; besides which they form the nodular masses of the solitary and agminated glands. It is also known that they are found extending between the columnar epithelium-cells which line the intestine, sometimes in considerable number. Since this is the case, and since,

moreover, they are amœboid and capable of altering their shape and position, and of taking up particles with which they may come in contact, I was long since led to infer that they might probably be regarded as active agents in effecting the transference of fat-particles from the columnar epithelium to the lacteals. This inference was based upon the following experiment, which I have often repeated. An animal is killed during digestion of food containing fat and whilst absorption is freely proceeding. On examining the small intestine it is found that the columnar epithelium-cells, the lymph-cells of the mucous membrane, and the lacteal vessels, are all occupied by particles of fatty matter. This fact can be readily substantiated by the mere examination of the fresh tissue, but it becomes strikingly manifest if a piece of the mucous membrane is placed in a 1 per cent. solution of osmic acid for a few hours, and after subsequently macerating in water for two or three days, is broken up with needles in a drop of glycerine. The fat-particles being now stained black are very conspicuous, and it is easy to recognise their presence in the epithelium-cells, where they are often of considerable size, and in the lymph-cells, where they are generally small.

Sections of the intestinal mucous membrane, and especially longitudinal sections of the villi, also show the fat-particles in the epithelium-cells, in the lymph-cells, and in the lacteals, but none in any other structures. The inference seems, therefore, unavoidable that the amœboid lymph-cells are the carriers of the fat-particles.

Nor is this view in itself an improbable one or without analogy. For the intussusception of particles is one of the most characteristic phenomena exhibited by amœboid cells, which will carry such incepted matters along with them in their slow movements from place to place. When vermilion is injected into the blood-vessels, the particles of pigment are taken in by the white corpuscles of the blood, and when these emigrate from the vessels the particles are carried with them, and may thus reach the radicles of the lymphatics. In the alveoli of the lung and in the smaller bronchial tubes, amœboid cells are constantly to be found filled with inhaled carbon particles which they have taken up from the mucus by which these particles have been intercepted. These cells are seemingly white corpuscles which have emigrated from the blood-vessels, and they pass back again into the lung-tissue between the epithelium-cells of the alveoli, carrying with them the fine particles of carbon, and eventually also reach the radicles of the lymphatics in which the carbon is deposited. Nevertheless, the view in question has not until lately met with any general acceptance, probably partly owing to the fact that contrary statements regarding the path of fat-absorption have been made by distinguished authorities, partly because it seemed to be an isolated instance of the participation of amœboid cells in absorption.

But this is no longer the case. In very numerous instances it has now been shown that amœboid cells are actively concerned in the transference and assimilation of nutriment. Observations on this point have chiefly been made upon animals low down in the scale of organisation, but have not been confined to these. For it has been demonstrated that a similar process occurs also in some fishes, whilst, as regards both normal and pathological tissue-absorption, strong evidence has been adduced in favour of the active agency of lymph- or white blood-corpuscles in the production of these changes.

The views of physiologists upon the subject of absorption would seem, in fact, to have taken a fresh direction, and the importance of the part played in the process by amœboid cells is coming to be very generally recognised. This being the case, I have been led again to turn my attention to the question of intestinal absorption in Vertebrates, considering it with reference not only to the mode of passage of fat into the lacteals, but also to the possibility of other alimentary substances being conveyed in a similar manner. With this object I have devoted a considerable amount of time at intervals during the last two or three years to a renewed investigation of the subject, with the assistance of a grant derived from the Government Grant Fund. The principal results arrived at in this investigation I propose now briefly to lay before the Society.

By far the most important of these results is the establishment of the fact that during absorption of food from the intestine the lymph corpuscles migrate in large numbers into the lacteals, and these for the most part become disintegrated and dissolved in the chyle. This is the case not only after a meal containing fat, but also after feeding with substances devoid of that alimentary principle; it is, therefore a phenomenon of general occurrence during absorption, and the carrying of fatty particles into the lacteals after a meal containing fat by the immigrating leucocytes, must be regarded as merely incidental to a more general function.

The number of leucocytes which thus pass from the tissue of a villus into its lacteal, is often so great that the blind end of the villus may be almost blocked by them. Lower down, however, *i.e.*, nearer the attachment of the villus, there are only very few to be seen, and some of these are only the more persistent nuclei of corpuscles, the protoplasm of which is already dissolved. Others are seen swollen and partially disintegrated, and, indeed, in preparations in which the cells have been fixed by the action of osmic acid, every stage of disintegration may be traced.

This immigration and solution of numerous leucocytes in the contents of the lacteals must be the means of conveying a large amount of proteid material, derived from their dissolved protoplasm and nuclei, into the chyle. And any other material which may be mechanically

or otherwise incorporated with their protoplasm must also be set free. In this way the fatty particles which they contain during absorption of a meal containing fat become released and suspended in the chyle, and it is probable that amyloid matters are also in part thus conveyed to that fluid.

The cause of the solution of the immigrated leucocytes in the chyle of the commencing lacteals is not easily discovered, but it is not difficult to imagine more than one way in which it might be brought about. It is known that a solution of peptones when added to the blood or lymph, produces disintegration and solution of many of the white corpuscles. And it is by no means unlikely that peptones which have passed by diffusion into the lacteals may produce a similar effect upon the immigrated amœboid cells. Moreover, after the blood is drawn many of the white corpuscles undergo solution in the plasma in consequence of changes in its physical or chemical condition which are inappreciable to the most delicate tests. And an increase of alkalinity in the blood which fails to produce any change whatever in the easily influenced coloured corpuscles will speedily produce the disintegration of most of the colourless cells.

As to the origin of these immigrant cells, it may be regarded as certain that they have passed inwards from the epithelium. Leucocytes are constantly seen, often in considerable number, between the epithelium-cells of the villi (where they may even lie close to the free surface), and also between the fixed ends of the epithelium-cells and the basement membrane, as well as in the tissue of the mucous membrane itself. In fat-absorption all these leucocytes, as well as those which have passed into the lacteals, contain fatty particles, and these particles are not found elsewhere in the tissue of the mucous membrane. Since there is no continuity of the protoplasm of the lymph-cells one with another, the fat-particles in those which are more deeply seated must have been carried along by the amœboid movements of the cells which contain the fat. In this case, then, it seems scarcely possible to come to any other conclusion than that the fatty particles are taken up by the leucocytes from the epithelium-cells, and perhaps in part directly from the intestinal cavity, are conveyed to the lacteal, and there become set free by the breaking down of the carrying cell.

The presence of the fat thus serves to trace the course of the leucocytes in the villi. But there is no reason to believe that their course is at all different even in the absence of fat, for they are still seen between the epithelium-cells, and they still pass into the lacteals. It is fair, therefore, to assume that they play precisely the same part whatever the nature of the aliment.

A further question as to the origin of the leucocytes involves the explanation of the production of a constant succession to take the

place of those which have passed into the lacteal. With regard to this, it may safely be affirmed that they are continually undergoing multiplication by division. No one has, so far as I am aware, ever doubted that the lymph-corpuscles are thus capable of renewal, although it has been generally believed that their mode of division is *direct*, *i.e.*, unaccompanied by those peculiar changes in the nucleus which have been termed karyokinetic or karyomitoic. This has, however, been settled by Fleming, who has shown that the cells of lymphoid tissue multiply abundantly by karyomitosis. This multiplication by division will apply to the leucocytes which lie between the epithelium-cells as well as to those in the mucous membrane proper. It is doubtful whether there is any addition caused by emigration of white corpuscles from the blood-vessels; certainly it is not by any means considerable.

It is a most important question whether any amœboid cells are produced by division of the columnar epithelium-cells. That these cells do divide is certain; in confirmation of other observers I have seen abundant evidence of the occurence of karyomitoic figures in them; but I am not yet able to say what is the result of such division, although I am strongly disposed to think that they may give origin to amœboid cells. Several instances can be cited of the origin of mesoblastic amœboid cells from hypoblastic epithelial cells in the embryo, which seem to favour this view.

Taking for granted that the leucocytes multiply by division, the newly produced daughter cells will naturally be relatively small with scanty protoplasm. Being freely supplied with nutrient material during the absorption of aliment, either directly from the intestine or indirectly from the columnar epithelium-cells (which are probably the primary agents in effecting absorption from the intestinal cavity), their protoplasm probably in part assimilates, and in part stores up this material and rapidly grows. Wandering now towards the centre of the villus (urged it may be by the stream of fluid which is passing at this time in that direction) the cells enter the lacteal, and there become dissolved, and again set free not only the proteid matters which they have assimilated and converted into protoplasm, but also all other material, whether in the form of definite particles or not, which may have been taken up in addition and carried by them into the lymphatic vessel.

The preparations upon which the observations and inferences which are here briefly recorded are based have been made chiefly from mammals, especially from the rat, in which the shape of the villi and the size of their lacteals render it relatively easy to obtain successful sections. Many, however, are from the frog, in which the facts can be very clearly made out on account of the size of the elements, and in which also the comparative slowness of the process of absorption

enables one more readily to study it in its different stages; although, on the other hand, the number of leucocytes which are passing into the lacteals and there undergoing disintegration is much less at any one period than in the mammal.

A fuller account of the subject, furnished with illustrations, and containing the necessary references to other articles dealing with the same question, will appear in the forthcoming number of the "Monthly International Journal of Anatomy and Histology."

January 29, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On some Physical Properties of Ice and on the Motion of Glaciers, with special reference to the late Canon Moseley's objections to Gravitation Theories." By the Rev. COUTTS TROTTER, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor STOKES, Sec. R.S. Received December 22, 1884.

I. *Introductory.*

It will be remembered that in a paper "On the Descent of Glaciers," read to the Royal Society on the 19th of April, 1855 ("Proc. Roy. Soc.," vol. 7, pp. 333—342; "Phil. Mag.," vol. x, pp. 60—67), Canon Moseley proposed a new theory to account for the phenomena of glacier motion. The theory was suggested, as is well known, by the observation of the gradual descent of a sheet of lead resting on a roof of moderate slope, and exposed to considerable diurnal variations of temperature. According to it the descent of a glacier is due to the alternate expansion and contraction of the ice in the direction of the length of the glacier under the influence of varying temperature; gravity assisting the downward and opposing the upward movement of the mass.

This paper was effectively answered by Forbes ("Proc. Roy. Soc.," vol. 7, p. 412), and the theory has never been accepted by persons

conversant with glaciers. The fatal objection taken by Forbes was that in a glacier, considered as a whole, there is no such variation of temperature, and consequently no such alternate expansion and contraction of the ice as Moseley's theory presupposes. The question of the temperature of the interior of a glacier will be considered further on. It will be sufficient to state here that theory and experiment lead alike to the conclusion that the variations of temperature due to the alternations of day and night, and even of summer and winter, are local and superficial only, the great working mass of the glacier, as it may be termed, being at a sensibly constant temperature. This fact had been brought out in the controversy which arose out of Charpentier's "dilatation" theory of glacier motion.

The late Mr. W. R. Browne, who recently came forward as a defender of Moseley's hypothesis ("Proc. Roy. Soc.," vol. 34, pp. 208—217), appeared to admit this fact, but argued that the upper layers of ice in expanding and contracting may drag with them the lower layers, or at least cause the upper layers to shear over the lower ones. Without discussing the possibility of such action in the general case, it may be sufficient to make two remarks.

(1.) As the motion of a glacier is continuous during the year, it must on Canon Moseley's hypothesis be caused by the diurnal as distinguished from the annual variations of temperature. Mr. Browne quotes Dr. Rae as saying, "We know that ice 2 or 3 feet or more thick contracts very considerably in a few hours by a sudden fall of 15 or 20 degrees of temperature." Now the cold of a summer's night in the Alps is far less intense than that of a "cold snap" in the American north-west, while it is very difficult to believe that even a very considerable expansion or contraction of the upper 2 or 3 feet of a glacier can affect the motion of a mass several hundred feet thick.

(2.) In many of the glaciers which move most rapidly the upper layers of ice are intersected by a system of transverse crevasses at short intervals. These extend so far as effectually to prevent any interaction between portions of the surface layer at any considerable distance from one another.

Moseley did not make any satisfactory answer to the objections brought against his theory, but in 1869 and the following years he put forward a somewhat formidable objection to the current gravitation theories of glacier motion. His paper was read to the Royal Society on January 7, 1869 ("Proc. Roy. Soc.," vol. 17, p. 202; "Phil. Mag.," vol. xxxvii, p. 229), and was followed up by a series of communications to the "Philosophical Magazine," of which the most important are to be found in vols. xxxvii, pp. 363—370, and xxxix, pp. 1—8. The gist of the objection is that the resistance of ice to shearing is many times greater than the shearing force

which can be produced in a descending glacier by gravity; and that therefore the shearing which the measurements of Forbes and others have shown to be an essential part of the motion of a glacier cannot be produced by gravity alone.

Mr. W. Mathews ("Alpine Journal," vol. iv, pp. 411—427), Mr. J. Ball ("Phil. Mag.," vol. xl, pp. 1—10), and others replied to Canon Moseley, but none of the answers were altogether complete, though both the above-mentioned authors clearly pointed out the weak point in Moseley's argument.

Moseley's objection is no doubt decisive if the shearing strength of ice as deduced from his experiments represents even approximately the resistance to shearing under the actual circumstances of glacier motion. But, as Mr. Mathews pointed out (*loc. cit.*, p. 426), the time during which the shearing force acted is an important element in the experiments. Moseley found ("Phil. Mag.," vol. xxxix, p. 8) that when the shearing took place in three minutes the shearing strength was about 118 lbs. per square inch, while when the operation lasted thirty minutes the result was about 112 lbs. per square inch. In one experiment the ice sheared in thirty-six minutes under a force of about 98 lbs. per square inch. Mr. Mathews pertinently remarks, "I am curious to know what weight would have sheared the ice if a day had been allowed for its operation." I have attempted to answer a question of this kind in the experiments recorded in the present paper.

In the section of the paper which immediately follows I have given a short notice of the most important recent experiments upon a small scale which bear upon the question of the viscosity of ice. In Section III I have described my own experiments on the subject. In Section IV I have dealt with the objection to the viscosity of ice drawn from its supposed inextensibility. In V I have discussed a novel argument of Mr. Browne's in support of a high value of the shearing strength of ice. In VI I have discussed the question of the probable temperature of different parts of the glacier, and of the significance of the "Bergschrund;" while VII contains a few concluding remarks upon the general drift of the paper.

II. *Previous Experiments bearing upon the Viscosity of Ice.**

Since the date of Canon Moseley's papers a good many experiments have been published which tend to show that ice will change its form

* By viscosity, I understand the property in virtue of which bodies change their form under the continued influence of forces which would be insufficient to cause an immediate change of form of similar amount. Probably nearly all, if not all, substances have more or less viscosity; but by a viscous solid may be understood a substance in which a large change of form is produced by adequate forces in a time which is neither exceedingly small nor enormously great.

under the influence of moderate forces applied continuously for a considerable time. In some of these, small bodies, mostly metallic, were forced into or through masses of ice. These experiments may be set aside as irrelevant. They were, or may have been, simply instances of the melting of ice through the lowering of the freezing point by pressure, and of subsequent freezing where the pressure is less. In fact some of the most striking results have been very properly published as examples of this action (*cf.* "Nature," vol. v, p. 185; vi, 396). The remaining experiments are for the most part experiments on the bending or twisting of masses of ice either under the influence of their own weight only or by means of superimposed weights of moderate size. Bianconi seems to have made some experiments of this kind as early as December, 1866, but he did not publish his results until 1871, when his early experiments, as well as others made in January and February of that year, were published in the "Memorie della Accademia di Bologna," Serie 3a, vol. i, pp. 155—166. In the meantime Mr. W. Mathews had made and published some interesting experiments in the winter of 1869–70 ("Alpine Journal," vol. iv, p. 426; "Nature," vol. i, p. 534). In these the influence of temperature was very marked. The middle point of a plank of ice 6 feet long and $2\frac{5}{8}$ inches thick, the ends of which were supported, sank 7 inches in about as many hours during a thaw. A somewhat thinner plank of the same length sank only about $3\frac{1}{2}$ inches in nearly three days of frosty weather. Results similar in kind were obtained by Tyndall (with glacier ice) ("Nature," vol. iv, p. 447), Pfaff ("Phil. Mag.," vol. l, p. 335), and others. Pfaff quotes Kane as having been the first to make such observations, but gives no reference.

These experiments show conclusively that the continuous action for a considerable time of comparatively small forces will produce effects upon ice which the same forces are quite incapable of producing in a short time, but they do not necessarily throw much light upon the actual processes which take place in a glacier.

When a glacier is descending in the usual river-like manner in a straight and uniform bed and down a uniform slope, the central part moving most rapidly, and the parts nearest to the bottom and sides of the channel being retarded by them, it is clear that the motion must take place by the ice shearing along cylindrical surfaces whose generating lines are parallel to the direction of flow of the glacier. In Canon Moseley's experiments, and in those of my own which are recorded below, the motion is essentially the same, the shearing taking place, approximately at all events, uniformly in one plane. On the other hand, when a beam of ice sags under the influence of its own weight the motion is much more complicated. Assuming that there is nowhere any change of density, but that the transformation takes

place exclusively by shearing, the direction as well as the extent of the shear will vary from point to point, and a simple observation of the flexure of the beam will not give a direct measure of the shearing strength of ice at any point. Moreover, the experiments were all made either during frosty weather, when the temperature of the ice experimented upon was variable, and the conditions therefore very different from those which obtain in the interior of a glacier, or else in a warm atmosphere while the ice was thawing rapidly, and the experiments could not be carried on for any considerable time under unchanged conditions.

III. *New Experiments.*

Under these circumstances it seemed desirable that fresh direct experiments on the shearing strength of ice should be made under conditions differing as little as might be from those under which ice actually shears in the interior of a glacier, and it occurred to me that such experiments might be advantageously made in one of the artificial grottoes which are now excavated year after year for the benefit of tourists in several of the more accessible Swiss glaciers. It seemed that it would be possible in this way to carry out experiments upon glacier ice at a nearly uniform temperature of about 0° C., and under conditions as nearly resembling the interior of a glacier as we can hope to attain to in experiments on hand specimens of ice.

I accordingly spent part of the long vacation of 1883 at Grindelwald, and made a series of experiments in the grotto on the right bank of the lower glacier, in order to see whether I could obtain direct evidence of shearing under the influence of forces comparable with those which Canon Moseley admits to be capable of being produced by the action of gravity in a moving glacier.

Situation of the Experimental Grotto.

The entrance to this year's grotto was at the base of a cliff of ice which I estimated at about 25 metres high. It was, by a rough barometrical comparison with Grindelwald, about 1275 metres above the sea-level, and about 75 metres above and perhaps 300 metres distant from the place where the glacier, now reduced at this point to a comparatively narrow tongue of ice, plunges into a deep and narrow gorge.

At the time of my arrival in Grindelwald (June 27) the grotto, the floor of which sloped slightly upwards, penetrated for about 18 metres nearly in a straight line, making an angle of about 55° with the face of the ice cliff, and then for about 8·5 metres further in a direction making an angle of about 3° only with the face of the cliff. It was a gallery nearly 2 metres high, and rather more than a metre wide.

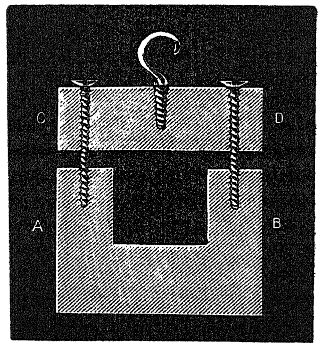
I had a lateral gallery excavated to contain my apparatus at the extremity of the main gallery, and on the side opposite to the edge of the glacier. This was of the same height as the main gallery, and about 3 metres long. The entrance was of the same width as the main gallery, but the end was somewhat wider, perhaps 1·7 metre. This lateral gallery was shut off by a rough door of planks from the main gallery, which was afterwards extended for a few metres beyond the entrance to the side gallery. The place in which my experiments were conducted was therefore some 18 metres from the edge of the glacier, 25 or 30 metres below its upper surface, and probably at least an equal distance above its bed.

There were some longitudinal crevasses of considerable size on the lower part of the glacier, and it is probable that one of these was not very far from the experimental gallery. I infer this from the fact that there was perceptibly more light in the side gallery than in the main gallery at the point where the side gallery branched off from it, and the light increased sensibly during the time (rather more than five weeks) that my experiments lasted. I attribute this increase of light to the melting of the side of a crevasse, and am disposed to estimate the distance from the end of the experimental gallery to the nearest crevasse at from 5 to 10 metres. After the first few days I kept maximum and minimum thermometers on the wooden frame which supported my apparatus. The extreme readings were 0° C. and 2° C. The temperature of the air and the gallery must have been raised from time to time by the presence of myself and others, particularly when a light was used, but for the greater part of the time the temperature of the air can scarcely have differed sensibly from 0° C.

The pieces of ice used in my first experiments were part of the material excavated from the side gallery. In the last I used a block hewn out expressly from the interior of the glacier. I always chose the most solid and transparent, and therefore presumably the hardest and strongest, pieces I could find. The blocks hewn by the pick were cut roughly to the required size and shape with a coarse-toothed saw; they were finished with a carpenter's plane set so as to cut a rather coarse shaving.

The apparatus used was in principle the same as Canon Moseley's. My apparatus was, however, disposed symmetrically so as to produce a shear in two parallel planes simultaneously. I was thus enabled to simplify the arrangements for fixing the apparatus, and to make my measurements more easily and accurately. Each of three pieces of board, AB (fig. 1), had a square notch cut in one side, so that when the short bar, CD, was fixed in its place by two screws, the whole formed a block with a square hole in its centre. A hook by which the

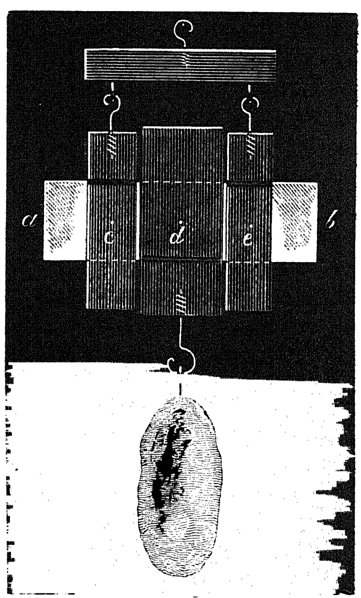
FIG. 1.



whole could be hung up was screwed into CD. This is drawn for distinctness sake in the plane of the board. It was actually in a plane at right angles to that of the board (see fig. 2).

Two of these blocks were suspended from a cross-piece (see fig. 2).

FIG. 2.



The distance of the hooks on the cross-piece was so adjusted that there was just room between the two hanging-blocks for the third block to move freely up and down. A square bar of ice, *ab*, was then

passed through the three square holes, a weight was hung on to the middle block, and the whole left hanging up in the grotto. Thin plates of cork were interposed between the ice and the wood where the pressure was greatest. At the close of the experiment the weight was removed, and the wooden blocks with the bar of ice removed to the edge of the glacier for examination. The weights used were stones taken from the moraine, the weight being ascertained in each case by a Salter's spring balance.

As the shear to be observed would probably be small, and some melting, at least of the surfaces exposed to pressure, was to be expected, it was necessary to fix fiducial marks in the substance of the ice. Several forms of wooden and metal plugs and pins were tried, but did not remain sufficiently firmly fixed to make accurate measurements possible. I ultimately obtained satisfactory results by boring three small cylindrical holes in a horizontal plane at about the points marked *c*, *d*, *e* in fig. 2

A small American twist drill about 2 mm. diameter, fitted to a carpenter's brace, made very uniform holes with sharp edges which, under favourable conditions, were very permanent and admitted of accurate measurements being made. Before boring the holes I applied the edge of a steel straight-edge to the ice. This melted a narrow straight groove in the line *a*, *b*, and by taking care to keep this line in the median plane of the body while boring, it was not difficult to secure that the axes of the holes should be very approximately in one plane, so that no appreciable error would be caused if the surface of the bar melted or had to be reworked before the straight-edge was reapplied at the close of the experiment, and the displacement of the middle hole noted. My working place on the moraine was not very convenient, and it was not quite so easy as it looked to apply a straight-edge accurately to the edge of the holes on the slippery surface of the melting block of ice, but a little experience taught me the necessary precautions, and in my later experiments the measurements agreed very well together and appeared to be quite satisfactory.

In my first experiments the middle block was rather more than 5 cm. thick, the outer blocks about 2·5 cm. The apertures were rather more than 5 cm. square. As it was not necessary to drive home the screws which attached block CD to AB (fig. 1), it was possible to take in a bar 5 cm. deep as well as the cork plates.

The following preliminary experiments may be recorded:—

(1.) July 5, bar 5 × 4·7 cm., weight 13·5 kilos. Examined July 9, shear estimated at ·05 cm., but measurements not quite trustworthy, melting slight, not measured. Shearing force at beginning of experiment 288 grm. per sq. cm.

(2.) July 9, bar 5 × 4·5 cm., weight 25 kilos. Examined July 14; the bar had apparently not been quite rectangular; there had been

sensible to the eye), its surface must be extended by more than a metre for every 60 metres of vertical thickness of the glacier. And it is much the same with the system of diagonal crevasses due to differential motion. It is true that the daily linear extension required to prevent fracture is in such cases usually very small, but a small extension of an inelastic mass like that of a glacier would seem to require molecular displacements which are much more complicated than those involved in a slight yielding of an elastic solid. If no yielding were possible, systems of diagonal crevasses would be as universal as differential motion. What is really proved by the often quoted observation of Tyndall ("Glaciers of the Alps," 1860, pp. 317—18), as to the extreme narrowness of crevasses when they are first formed, is that ice is not capable of any appreciable *elastic* extension before it gives way. Such want of elasticity is often very characteristic of viscous as distinguished from strictly solid bodies.

I believe, therefore, that the weight of evidence tends to show that ice at or about 0° C., is just as truly viscous as pitch or sealing-wax at temperatures at which they are brittle but yet capable of yielding to the continuous application of a very moderate force. The viscosity of ice, however, probably diminishes very rapidly with the temperature. M. Person ("Comptes Rendus," xxx, pp. 526—528), quoted by Forbes, infers from experiments on the latent heat of fusion of ice that ice begins to soften at about −2° C. Ice just before it melts appears to be quite soft. When during the preparation of the bars for the experiments described in this paper, I had occasion to interrupt for a few minutes my work in the comparatively warm atmosphere of the moraine, I was repeatedly struck by the difference between the sensation caused by the first one or two cuts with the plane, and that experienced after half a millimetre or so of the melting surface had been removed. Many persons must have noticed the way in which a piece of ordinary clear ice which has been wrapped in a coarse linen cloth on a summer's day adheres to the cloth, and has the marks of the threads impressed upon it. It may be said that this is an instance of the lowering of the melting point by pressure and subsequent freezing; but when it is considered that the phenomenon may be observed when 1 lb. per square inch is a liberal estimate of the pressure exerted, so that the lowering of the freezing point would be about ·0005° C., and the melting of 1 mgrm. of ice would produce sufficient absorption of heat to cool down about 320,000 mgrm. of ice to the new melting point, it seems more natural to suppose that the ice just before it melts passes into a soft somewhat sticky condition like that of sealing-wax at about 100° C. It is scarcely surprising that this condition is not more often obvious, as it is difficult to touch the melting ice with anything which will not convey to it heat enough to produce complete liquefaction.

The supposition that, while ice at 0° C. is sensibly viscous, the viscosity diminishes rapidly with the temperature, is in complete accordance with the facts of the changes which take place in a glacier during the winter. The terminal melting ceases, but the advance of the end of the glacier into the valley is very slow, and possibly ceases altogether in the depth of winter. Higher up the forward movement of the surface continues, though at a slower rate than in summer, and though the glacier does not lengthen much in winter, it thickens considerably, and the surface rises, often through many feet, so as to make up (in a glacier which is neither increasing nor diminishing) for the enormous surface waste of the summer. This is exactly what we should expect if the great working mass of the glacier retained the same mobility in winter as in summer, while the surface layers and the extremity had their resistance to change of form very greatly increased. The whole movement of a glacier in winter is closely parallel to that of a lava stream when it is beginning to cool, when the outer crust and the terminal portion have become solidified while the great mass remains semifluid; the stream continues to advance, but only slowly, while the lower portions increase in thickness like those of a glacier in winter.

V. *Mr. Browne's Argument from Ice Cliffs.*

Mr. Browne in the paper which has been above referred to ("Proc. Roy. Soc.," vol. 34, p. 210) brings forward an additional argument in favour of a large shearing strength of ice. He calculates the shearing strength necessary in order that a vertical ice cliff of a certain height may be able to stand, and finds about 30 lbs. per square inch to be the minimum strength consistent with the existence of ice cliffs 300 feet high. The argument would be perfectly sound if for "stand" we read "stand permanently," but as it is put it is liable to the same objections as Canon Moseley's direct experiments upon shearing.

Crevasses 300 feet deep are said to exist, but an individual crevasse is by no means a long-lived structure. When the bed of a glacier widens suddenly below a projecting vertical face of rock, we sometimes find a cliff bounding the glacier for some little distance below the projection. It never, however, extends very far, and when the glacier-bed widens gradually the ice spreads out so as to fill the wider bed without any cliff being formed at all.

The spreading out of a glacier like the Rhone Glacier where it emerges from a gorge on to a comparatively open space, which Mr. Browne strangely enough quotes (*loc. cit.*, p. 215) as favourable to Canon Moseley's view, is in itself a convincing proof that ice at 0° C. will not stand permanently in a vertical cliff of any considerable height; it gives way gradually, but still it gives way.

VI. *Temperature of Glaciers, and significance of the "Bergschrund."*

Throughout this discussion it has been assumed that the temperature of the great mass of a glacier does not differ much from 0° C. It seems desirable to examine the reasons for this assumption, to consider whether it is liable to any important exceptions, and if so to deduce their consequences. I do not know that there is in what follows much that is substantially novel, or that is seriously controverted, but it may be worth while to formulate in a systematic shape what has hitherto been contained in a number of isolated remarks or suggestions scattered through glacier literature.

It is obvious that no part of a glacier can be at a higher temperature than 0° C., for the simple reason that ice or snow cannot exist at a higher temperature.

The snow which is the ultimate source of the glacier ice falls at temperatures which may vary from 0° C. downwards. Snow weather is, however, not usually very cold weather, and it is probable that falls of snow at a very low temperature are exceptional.

The surface of a glacier may sometimes become very cold by radiation, but ice is a very bad conductor of heat, and dry snow is still worse. There is no reason to suppose that the cold of a summer night can penetrate many inches into the glacier, and it is improbable that the cold of winter can be felt at more than a few feet below the surface, since the whole glacier is in winter covered with snow. On the other hand, on a warm summer day a very large amount of heat is absorbed in the upper layers of a glacier, and is carried into the lower layers by the water which sinks into the mass. It is clear that so long as water at 0° C. sinks through snow or névé at 0° C., it will descend unchanged, neither giving up nor receiving heat. When it meets with snow or névé at a lower temperature, it will be partially frozen, and its latent heat of freezing will warm the snow or névé up to 0° C. Forbes found ("Eleventh Letter on Glaciers," Edinburgh, "New Philosophical Journal," October, 1846, reprinted in "Occasional Papers," pp. 169, 170) that the mean daily waste of the surface of the Mer de Glace opposite the Montanvert during hot weather in July and August, 1846, was 3·62 inches per day, while higher up between l'Angle and Trelaporte it was 2·73 inches. Now as the latent heat of fusion of ice is about 80 calories, and the specific heat of ice about ·5, the heat required to melt a cubic inch of ice would raise about 160 cubic inches through 1° C. No doubt in the upper regions of névé the heat absorbed is a good deal less; the air is colder, and a smaller proportion of the solar radiation enters, and is absorbed by, the glacier. On the other hand, in these regions the whole of the water produced by melting must sink into the glacier,

and its latent heat of fusion must be used in warming the glacier so long as any stratum is below 0° C.

The amount of heat which reaches the glacier from the interior of the earth is no doubt insignificant in amount when compared with that which reaches the upper surface of the glacier, but it is important as necessarily warming and eventually melting the lowest stratum of the glacier. Forbes ("Travels in the Alps," 1843, p. 364) quotes an estimate of M. Elie de Beaumont, according to which the annual flow is sufficient to melt about $6\frac{1}{2}$ mm. of ice. This would be sufficient to raise about 1·040 metres of ice 1° C. Considering that the winter's cold cannot possibly penetrate to the bed of the glacier, this is quite sufficient to ensure that the lowest stratum shall be at the temperature of about 0° C. I cannot lay my hand upon any more recent estimates of the flow of central heat from the earth, but I believe that M. de Beaumont's would be regarded as correct at least so far as the order of magnitude is concerned.

We may say then with confidence that the ice which rests directly upon the bed of the glacier is at the temperature of fusion corresponding to the pressure at the spot, *i.e.*, usually very slightly below 0° C. Neglecting these small variations, and leaving out of consideration for the present the extreme upper portion of the glacier, we may say that the surface of contact of a glacier and its bed is part of an isothermal surface of 0° C. The upper surface of the glacier during a summer's day, when melting is everywhere going on, must also be an isothermal surface of 0° C. At night this upper isothermal will descend a few inches into the ice, and in winter perhaps a few feet. The portions above this upper isothermal will be at temperatures below 0° C. Disregarding as before the small differences of melting point due to pressure, there can be little doubt that throughout the lower part of the glacier the whole space between these isothermal surfaces will be filled with ice at 0° C. In the upper portions of the névé it is possible that there may be one or more regions in which the temperature is below 0° C. These will be bounded by isothermals of 0° C., which in summer will necessarily be closed surfaces, except possibly at the extreme upper end of the glacier; in winter they may be in some places continuous with the cold region above the main upper isothermal of 0° C.

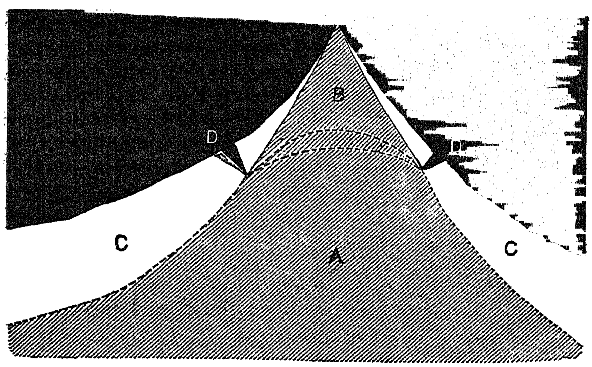
We may now consider the state of things near the extreme upper limit of the glacier.

In a large number of instances the névé basin which forms the upper reservoir of the glacier is bounded by a steep and comparatively narrow rocky ridge, against the lower part of which a relatively shallow mass of névé or snow rests at a high angle; the other side of the ridge is very frequently the boundary of another glacier. The mean temperature of the air at such elevations is low, and though the

rocks will in some places and at some times receive and absorb a large amount of solar radiation, they will also radiate freely into space. The amount of central heat flowing into such a wedge of rock through its comparatively narrow base cannot be large, and there can be little doubt that the mean temperature of the upper portions of such a ridge will be below 0° C. It seems clear that in such a case the isothermal surface of 0° C. which follows the common surface of the glacier and its bed, must somewhere leave the surface of the rock and strike across the ridge, so as to be continuous with the isothermal surface at the lower surface of the glacier on the opposite side. In such a case the most probable point for the isothermal to leave the surface of the rock will be about the place where the ridge begins to rise steeply. The winter isothermal of 0° C. within the rock will be below, but probably not far distant from the summer one.

Now in such a typical system of glaciers as I have been describing there is almost invariably a deep chasm in the névé at about the point at which I have supposed the isothermal to leave the surface of the rock. This chasm, which penetrates obliquely into the névé, and is often nearly continuous for long distances, is familiar in Alpine literature as the "Bergschrund."

Fig. 3.



In the annexed diagram, A is the rock of supposed mean temperature greater than 0° C.; B the rock of supposed mean temperature less than 0° C.; C the névé; D the "Bergschrund." The dotted lines give the trace upon the plane of the paper of the supposed winter and summer isothermals of 0° C.

I am not aware that any observations of the motion of the extreme upper portions of a glacier have been made, but I do not think that any one who is familiar with glacier scenery will have much doubt that the "Bergschrund" marks the limit between the moving and the

fixed parts of the glacier. It appears that the snow or névé above the "Bergschrund" does not move downwards as a glacier, but that the snow which falls upon it slides down from time to time in avalanches over the surface, usually choking up the "Bergschrund" in the spring. On the other hand, everything below the "Bergschrund" moves forward, slowly indeed but still moves forward, with the glacier. There is, however, no marked difference, at any rate in the conditions of the surface layers, between the snow or névé above and below the "Bergschrund," so that it seems at first sight paradoxical that the névé on the steep slope should remain at rest while that on the less steep slope below is in motion. I believe the essential difference to be in the temperature of the lowest stratum which is in contact with the rock. When the temperature is 0° C. the névé is soft and viscous, and therefore yields gradually even to a very moderate pressure. The forces acting upon the lower stratum of névé in the upper and steeper portions are greater, but the névé in contact with the rock is hard frozen and unyielding. No doubt the névé above the "Bergschrund" is usually of comparatively small depth, but even a comparatively thin layer of névé will advance slowly in true glacier fashion when it rests upon a gently sloping bed in the hollow of a mountain side, and so receives enough central heat to keep the lowest stratum in a soft condition. This is well seen in some of the so-called glaciers of the second order.

If the regions of névé below 0° C. which have been previously spoken of exist in the moving portion of the glacier, they may not improbably move on with very little change of form, riding as it were upon the warmer and softer substance below. Before they can reach the lower part of the glacier and be transformed by regelation into compact ice their temperatures must have risen to 0° C., and they cannot again fall below this temperature except close to the surface of the glacier.

VII. *Concluding Remarks.*

The general result of the foregoing paper seems to be that the fuller consideration of the physical properties of glacier ice leads to essentially the same conclusions as those to which Forbes was led forty years ago by the study of the larger phenomena of glacier motion—that is, that the motion is that of a slightly viscous mass partly sliding upon its bed, partly shearing upon itself under the influence of gravity. To say this is, however, by no means to deny the importance of regelation in the economy of a glacier. To regelation mainly we must attribute the gradual passage of snow through the form of névé into ice, the healing of crevasses, and the possibility of comparatively rapid and violent changes of form in portions of a

glacier in which unusually powerful forces may be supposed to be at work. Moseley's argument, however, seems to be decisive against the belief that the ordinary comparatively undisturbed descent of a glacier along a moderately sloping bed takes place by fracture and regelation. Moseley's value of the shearing strength of ice, which has been shown to be enormously too great as a measure of the resistance of ice to slow shearing, would appear on the other hand to be an inferior limit to the resistance to the shearing fracture which must precede regelation. Moseley has at any rate done good service by calling attention to the comparatively small intensity of the shearing force of gravity in the ordinary descent of a glacier. It would hardly have occurred to one fresh from the study of Forbes to look for evidence of the viscosity of ice in hand specimens exposed to moderate shearing forces in a laboratory.

II. "On the Structure and Rhythm of the Heart in Fishes, with especial reference to the Heart of the Eel." By J. A. McWilliam, M.D., Demonstrator of Physiology in University College, London. (From the Physiological Laboratory, University College, London.) Communicated by Professor Schäfer, F.R.S. Received January 14, 1885.

I. *On the General Arrangement and Structure of the Eel's Heart.*

The pulsation of the eel's heart can easily be seen externally on the ventral surface of the body a short way behind the pectoral fins. There are no rigid structures of any kind between the integument and the heart. When the very tough and resistant skin is cut through and the great lateral muscles are separated from each other, the pericardium is seen, loosely adherent to the surrounding tissues. The pericardial cavity being laid open, the various parts of the heart, abundantly lubricated with fluid, come into view. The organ is not freely suspended in the pericardial chamber, but is attached to the walls of that chamber by numerous and considerable bands, which vary in size and arrangement. The bands connected with the ventricle pass chiefly to the lateral and dorsal aspects of that part; they generally communicate with one another, forming a plexiform arrangement, and they tend materially to restrict the locomotion of the organ during systole. These bands are for the most part fibrous; they convey, however, large blood-vessels to the ventricle. These blood-vessels come from the dorsal part of the pericardial chamber and climb up in the fibrous bands on to the moving ventricle to

ramify on its surface. The partial fixation of the ventricle by the bands mentioned is probably in relation with this peculiar mode of blood-supply. There are generally two such vessels passing on to each side of the ventricle. These vessels are arterial; they are distributed to the *outer* part of the ventricular wall; they do not supply the whole thickness of the ventricular muscle. The greater part of the ventricular substance is spongy (like the ventricle of the frog) and is permeated by the venous blood passing through the heart; the outer part, however, is much more dense and compact in structure, and is supplied with arterial blood by the special system of vessels mentioned above. Veins are also seen upon the surface of the ventricle; they run upwards and backwards to terminate near the mitral orifice, opening there into the tubular vessel which connects the ventricle with the remaining parts of the heart.

The ventricle is also attached to the bulbus arteriosus, to the auricle, and to the sinus, by means of fibrous bands, which vary considerably in number and arrangement. A series of such connexions between the auricle and the ventricle exists around the mitral orifice.

The auricle is attached dorsally near the middle line by several short thick bands, which pass to it from the dorsal aspect of the pericardial chamber; there are also some slender threads, which pass to its ventral and lateral surfaces. The auricle is similarly connected to a slight extent to the sinus, and the sinus has some connexions with the lateral parts of the parietal pericardium.

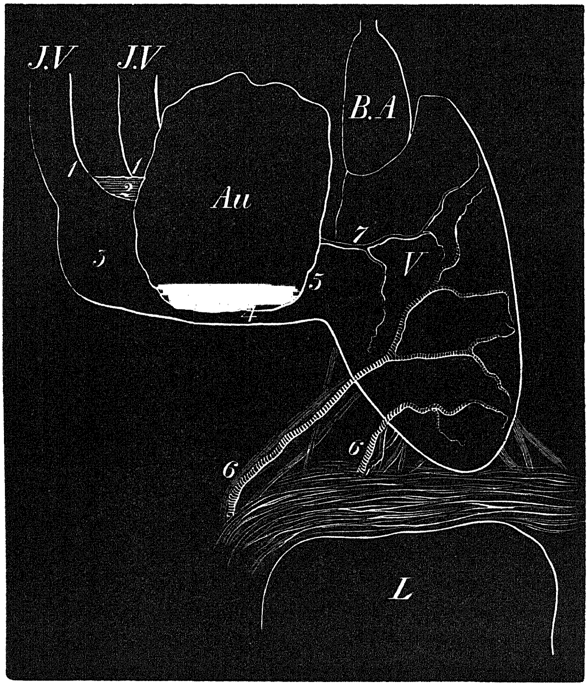
The sinus is placed dorsally, and is almost concealed by the overlying auricle. It receives, besides other vessels, the right and left jugular veins, which enter it from above.

Between the terminations of the two jugular veins, there is a part of the sinus marked off from the rest by a slight fold—best distinguished when the heart is well filled with blood. This part is physiologically distinct; it may be termed the "interjugular part of the sinus." It becomes continuous with the dorsal wall of the auricle.

The ventral wall of the sinus passes on to be directly attached to the ventricle at the mitral orifice. The proper auricular tissue does not form a complete chamber; it forms the lateral and dorsal parietes of a chamber, the floor of which is chiefly made up by the prolonged ventral wall of the sinus. (This prolonged ventral wall of the sinus corresponds to the "basal wall" described by Gaskell in the tortoise heart.) The auricle appears as a sort of appendage or diverticulum bulging laterally and dorsally from the prolonged ventral wall of the sinus. The auricle is not directly contiguous to the ventricle, but is separated from the latter by a short intervening tubular vessel, somewhat resembling the canalis auricularis of the mammalian fœtus.

This tubular vessel becomes continuous with the ventricle at its mitral orifice, which is placed a little below the middle of the dorsal aspect of the ventricle.

EEL'S HEART (diagrammatic).



J.V. Jugular Veins (right and left).
Au. Auricle.
V. Ventricle.
B.A. Bulbus Arteriosus.
L. Liver.
1. Ostial parts of Sinus.
2. Interjugular part of Sinus.
3. Main part of Sinus.
4. Basal wall of Auricular Chamber continuous with ventral wall of Sinus.
5. Tubular communication between Auricle and Ventricle (canalis auricularis).
6. 6. Arteries passing to Ventricle.
7. Veins on surface of Ventricle.

When the circulation is going on this disposition of the parts of the heart is rendered extremely obvious by the differences in colour presented by the various portions. The sinus is of a blue colour; the auricle is dark red; the ventricle is of a lighter red tint. When the ventricle is turned upwards, the blue ventral prolongation of the sinus

forming the floor of the auricular chamber is very apparent; and when the fibrous threads attaching the external surface of the auricle to that of the ventricle around the mitral orifice are divided, the blue intervening tubular vessel is plainly seen. The connexion of this vessel (canalis auricularis) with the ventricle is seen, under the microscope, to be chiefly established by connective tissue. There is, however, muscular continuity as well; for the ventricular substance and the muscular wall of the above-mentioned vessel (canalis auricularis) are connected by an extremely narrow and prolonged isthmus of muscular tissue.

II. *On the Spontaneous Rhythm of the Heart as a whole and of its Various Parts.*

The normal contraction of the heart begins by a distinct simultaneous beat in the right and left jugular veins, near their termination in the sinus. This spot may be denominated the "ostial part" of the sinus. Thence the contraction passes along the remaining part of the sinus to the auricle, and then to the ventricle. The mitral orifice being placed, not at the upper end of the ventricle, but a little above the middle of its dorsal aspect, the contraction of the ventricle begins at this part, and affects the middle portion of the ventricle before it passes over the upper and lower ends.

The normal order of contraction seen in the heart can easily be reversed by direct stimulation of the ventricle. Contraction then begins at the stimulated point and spreads over the whole heart; the ventricle first contracts, then the auricle, and lastly the sinus. This reversed mode of contraction usually ceases to be seen whenever the stimulation of the ventricle is discontinued; in some instances, however, it persists for a short time longer.

The passage of a constant current through the ventricle is sometimes able to cause that part to contract first, and so to lead to a reversal of the normal order of contraction of the parts of the heart.

During the continuance of the reversed mode of cardiac contraction, the circulation becomes much obstructed; the auricle and sinus are distended with blood. For the auricular beat occurs before the ventricular beat has ended, and the auricle is unable by its systole to force the blood into a contracted ventricle.

A regular contraction of the auricle leads very readily to a beat of the ventricle. A partial contraction affecting only the part of the auricle remote from the ventricle is not followed by a ventricular contraction; a partial contraction affecting the part of the auricle adjacent to the ventricle is usually followed by a ventricular beat, just as if a regular beat of the whole auricle had occurred.

But apart from the occurrence of a preceding auricular beat, a

ventricular contraction may occur in sequence to a sinus beat without the intervention of a contraction of the proper auricular tissue at all. This is evidenced by the state of matters observable in a mode of cardiac action which frequently presents itself as a result of certain conditions—especially nervous influences—to be afterwards described. The phase to which I refer is that in which contraction of the sinus, followed by contraction of the ventricle, occurs once or many times without any auricular contraction at all. Here the contraction is first seen in the sinus; after an appreciable pause it passes over the ventricle, the whole auricle meanwhile remaining perfectly motionless. Moreover, if the ventricle is made to contract first (*e.g.*, by direct stimulation), the contraction is after a short interval propagated to the sinus, the auricle being perfectly quiescent as before. And the presence of the proper auricular tissue is not at all necessary for the transmission of the beat between sinus and ventricle. For the whole of the auricle proper can be removed without interfering with the propagation of the contraction from the sinus to the ventricle—provided the direct anatomical connexion of the sinus and ventricle be left intact—that connexion which has been described as a prolongation of the ventral wall of the sinus to the ventricle (basal wall). In the absence of all the auricular tissue the ventricle continues to respond regularly to each beat of the sinus. That the ventricular beats here observed are really consequent on the preceding sinus beats—that the action of the ventricle is not an independent automatism, can be readily shown by detaching the ventricle from all connexion with the sinus tissue. When such is done the ventricle either stands still or goes on beating at a much slower rate than the sinus, and with a rhythm independent of that of the sinus.

It is obvious then that contraction may readily pass from sinus to ventricle, or from ventricle to sinus, without the intervention of any of the proper auricular tissue. And this condition seems to obtain not only in the heart of the eel but in that of many other fishes as well, *e.g.*, salmon, carp.

On the other hand, contraction can be readily propagated from the sinus to the ventricle, and *vice versâ*, through the auricular tissue, without the presence of the direct anatomical connexion (basal wall) between sinus and ventricle. For if this connexion be completely divided it will be found that the contraction can pass with the greatest regularity from the sinus over the intact auricle to the ventricle. And partial section of the auricle shows that a very slender strip of auricular tissue is sufficient to allow the transmission of the contraction to take place. A "blocking" of the contraction can easily be brought about by further section or by carefully applied pressure—just as Gaskell has found to be the case in the heart of the tortoise. And many of the conditions which Gaskell has described with refer-

ence to blocking in the tortoise heart seem to obtain in the eel's heart as well. If the means employed to bring about blocking have been kept within certain limits—if the section has not been carried too far or if the pressure applied has not been too great—the blocked condition usually passes off after a time, and the normal propagation of the contraction is again evident. The recovery of the tissue from the blocked condition can be materially accelerated by the application of the normal salt solution, and a similar beneficial effect is often apparent after the cardiac action has been arrested for a time by stimulation of the vagus nerve.

The normal sequence of the events constituting a cardiac beat is often in the course of prolonged experiments seen to become interrupted. The change which presents itself with the greatest frequency is a failure of the ventricle to respond to each beat of the other parts of the heart. Such failure often occurs without any loss of excitability in the ventricular tissue: the condition often seems to be one in which the propagation of the contraction from the auricle to the ventricle is interrupted. The ventricle then seems to remain quiescent, not because it is incapable of contracting, but because the contraction is not transmitted to it from the auricle and sinus. The failure of transmission seems to occur at the junction of the ventricle with the rest of the heart—at the mitral orifice. This fact is of interest when considered in relation with the peculiar character of the muscular connexion between the ventricle and the rest of the heart—a connexion which is established by means of an exceedingly narrow and prolonged isthmus of muscle substance. Whether or not this peculiarity in structural arrangement is the cause of the frequent failure of conduction at this part is a question on which it would be premature to make a decided statement.

A similar failure of the ventricular sequence can usually be brought about by repeatedly heating the whole heart or the sinus and auricle alone. Heat causes a great acceleration of the beat with a simultaneous enfeeblement. When the contractions of the auricle and sinus have thus been rendered extremely weak, a complete suspension of the ventricular action is commonly seen. When the auricular beats begin to recover their strength, the ventricle again begins to respond to each auricular beat, even though the auricular rate be still very rapid; the ventricle contracts in sequence to each auricular beat if the auricular beats are tolerably strong. The strength of the auricular beats seems to be a very important factor in regard to the question of the transmission of the contraction to the ventricle. And this statement is borne out by a number of facts—among others by a result which is often seen when the auricular beats are rendered excessively weak in consequence of their being elicited in rapid succession by artificial stimulation. In

such circumstances the ventricular action is often completely suspended until such time as the auricular contractions are slower and consequently stronger.

When failure of the ventricular sequence has occurred, the normal order of events can often be restored for a time—(1) by application of salt solution to the junction of the ventricle with the rest of the heart; (2) by passage of an interrupted current through the same part; or (3) by stimulation of the vagus nerve, leading to cardiac standstill, followed by a phase during which the normal ventricular sequence is restored. Whether this result of vagus stimulation is due to a direct beneficial effect on the tissues or to the rest which has been afforded to the various parts, is not at present clear.

Failure of the ventricular sequence seems to be at times associated with a depressed excitability of the ventricle itself.

All the parts of the heart when isolated from one another can manifest the property of automatic rhythmical action, though this property is possessed by the different portions of the organ in very different degrees. The excised ostial parts of the sinus go on beating at the ordinary rate of the heart's rhythm. The interjugular part when isolated exhibits (after a short pause) an independent rhythm which is slower than that of the ostial parts. The prolongation of the sinus to the ventricle comes next with regard to rhythmic power; then the auricle; and finally the ventricle. As regards the property of independent rhythmic contraction then, the various parts of the heart form a descending series, the highest term of which is the ostial part of the sinus; the lowest term is the ventricle.

The high rhythmic power possessed by the sinus constitutes it the leader in the series of events that make up the cardiac beat. Each contraction originating in the sinus of a normal intact heart spreads over the remaining parts of the organ, and thus leads to a rate of action in these parts identical with that in the sinus—a rate of action very much more rapid than they could exhibit in virtue of their own independent rhythmic power.

III. *Some Points in the Behaviour of the Heart with regard to the Results of Direct Stimulation.*

I shall here deal chiefly with the phenomena exhibited by the ventricle, reserving the consideration of the results obtained in the case of the auricle until the influence of the cardiac nerves has been discussed.

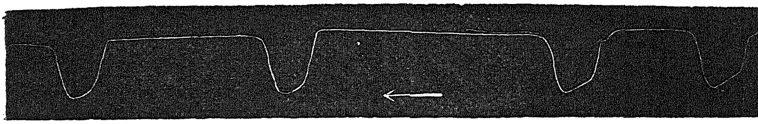
A single stimulation applied to the ventricle is followed by a single responsive beat. If the ventricle experimented on forms part of an intact heart, the result of a single excitation is to cause a reversed beat of the heart, the ventricle contracting first. And if the

circulation is going on, a peculiar dark bulging of the ventricular wall occurs at the stimulated part; this bulging occurs towards the latter part of the systole which results from the excitation. This phenomenon occurs with great constancy and in well-marked form in all cases where the circulation is intact and the ventricle is well filled with blood. It is not seen in the bloodless ventricle. The momentary or continued application of pressure may cause the same result, even though the pressure be too weak to act as a direct stimulus leading to contraction of the part where it is applied; the bulging may occur at the spot where pressure has been applied, even though the contraction of the heart occur in the normal fashion. The *rationale* of the phenomenon in question seems to be that in the area where a direct stimulus or even pressure insufficient to act as a direct stimulus has been applied, the muscular contraction is much impaired in efficacy, and as a result that area is unable to resist the high intra-ventricular pressure which occurs during systole. The area in question, therefore, becomes dilated with blood forced into it by the vigorous contraction of the rest of the ventricle; hence the dark bulging apparent on the surface. A somewhat similar bulging is occasionally to be seen in the ventricle of the frog as a result of strong direct stimulation.

The application of a strong direct stimulus (*e.g.*, an induction shock) to the ventricle is able to elicit a forced beat at almost any phase of the cardiac cycle. In a spontaneously-acting heart the occurrence of a forced beat from ventricular stimulation is usually followed by a prolonged diastolic interval before the appearance of the next spontaneous beat. In an intact and normally acting heart this diastolic prolongation observable on a ventricular tracing is not due directly to the properties of the ventricle; it is due to the conduct of the leading part of the heart—the sinus. For the rate of contraction of the ventricle of an intact heart is determined by the sinus—in which the contraction of the heart originates. The property of exhibiting a prolonged diastolic interval after a forced beat is not, however, peculiar to the sinus. It is equally well seen in the case of an isolated automatically-acting auricle or ventricle. A similar phenomenon has been described in the case of the entire frog-heart, and has there been attributed by some observers to an influence of the nervous mechanism.

The contraction which occurs after the prolonged diastolic interval is frequently of markedly increased size.

When the heart is intact and the circulation going on, the form of curve yielded by an artificially excited ventricular beat (resulting from direct stimulation of the apex of the ventricle) is quite distinct from that traced by a beat which occurs in normal sequence to an auricular beat; and the naked-eye characters of the beats obtained in the two instances are quite different.



Tracing shows difference in curves of normal and artificially-excited (reversed) ventricular beats. The two right-hand beats are spontaneous ones; the two left-hand beats were caused by direct stimulation of the ventricle with single induction shocks.

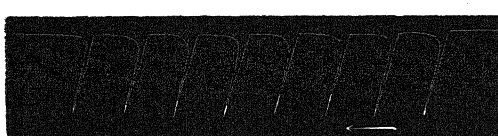
The muscular tissue of the isolated ventricle presents some characters that are similar to those of the frog's ventricle, and some that are markedly different.

I shall proceed to refer briefly to some points in the behaviour of the eel's ventricle with regard to

(1.) Single stimulations, electrical and mechanical.
(2.) Faradisation with strong and weak currents.
(3.) The constant current.

With regard to the effects of single stimulations on the quiescent ventricle, minimal stimulation is at the same time maximal—as in the frog's ventricle. Induction shocks obtained with one Daniell's cell in the primary circuit, when the secondary coil of the Du Bois Reymond's induction machine completely covers the primary coil, give the same strength of contraction as do shocks obtained when the secondary coil is 10 cm. removed from the first-mentioned position.

When the isolated quiescent ventricle is made to contract regularly by induction shocks at certain definite intervals (*e.g.*, 10 seconds) there is, as a rule, no progressive augmentation of the contraction force up to a maximum—no staircase of beats ("aufsteigende Treppe" of Bowditch), like that which is so conspicuously seen in the ventricle of the frog in similar circumstances. In the quiescent ventricle of the eel's heart the maximum beat is almost invariably obtained at once;



Tracing showing beats obtained from an isolated quiescent ventricle by stimulating it at intervals of 20 seconds with single induction shocks.

the beats elicited at regular intervals by a long consecutive series of shocks are almost always of exactly the same size, provided the interval between the successive shocks is sufficiently long to allow full recovery

of the muscular tissue from the effects of the preceding contraction. If the shocks follow each other too rapidly, the curves obtained assume the character of a descending series, resembling in the main a fatigue trace of an ordinary voluntary frog-muscle—at least as far as the progressive diminution in the size of the beats is concerned. When the ventricle has thus been reduced to a condition of fatigue, if the interval between the shocks be lengthened, the beats will for a time show a progressive increase in force, as more time is now allowed for the recovery of the tissue from the effects of contraction. Whenever the recovery from the fatigued condition is complete, the beats remain of fixed strength, showing no alteration whatever as long as the conditions under which the experiment is conducted remain constant.

Mechanical stimulation of the quiescent ventricle gives the same results.

Only in one ventricle (amongst a large number examined) have I seen any trace of the "beneficial effect of contraction." And in that case the ventricle had sustained considerable mechanical injury before the experiment was begun.

In order to test further the properties of the ventricular muscle with regard to the production of a staircase of beats in response to a regular series of stimulations, I have many times repeated the experiment on a quiescent ventricle in the uninjured state filled with normal blood. For this purpose the ventricle of a normally acting heart was rendered quiescent by stimulation of the vagus nerve—a proceeding which, by arresting the action of the other parts of the heart, leads to ventricular standstill without (as will be explained in the following pages) influencing in any way its contraction force. The standstill of the ventricle occurs simply as a result of the quiescence of the other parts of the heart from which the rhythmical contractions are normally propagated to the ventricle. An uninjured ventricle that has been rendered quiescent in this way is presumably in a more normal state than an isolated ventricle artificially fed outside the body.

Such a ventricle, excited at regular intervals by a series of mechanical or electrical stimulations, gives a series of beats of precisely equal strength, the commencing beats of the series being maximal, and differing in no respect from the subsequent ones.

With regard to the effects of faradisation upon the ventricular tissue, some remarkable results are obtained.

A surprising influence is found to be exerted by the *rate of interruption* in the faradising current.

It is well known that in the case of ordinary voluntary muscle when a slowly interrupted current is applied, the muscle contracts at the rate at which the shocks are sent in. And when a more rapidly interrupted current is employed, the muscle exhibits a more rapid

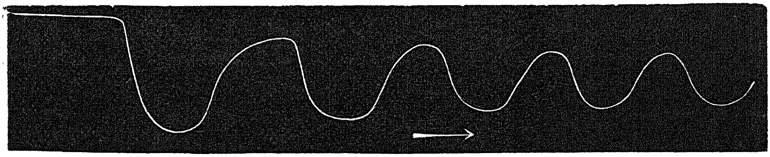
succession of contractions—in response to the more rapid series of shocks sent into it. In short, the rate of contraction depends on the rate at which the stimulations are applied; the quicker the series of shocks sent in, the quicker is the series of responsive contractions.

In the eel's ventricle the phenomena are of a strikingly different nature. For a slowly interrupted current is found to cause a much more rapid series of contractions than does a rapidly interrupted current of precisely equal strength. When a rapidly interrupted current is sent through the quiescent ventricle (of an eel's heart), the rate of contraction induced varies within certain limits according to the strength of the current employed. When a rapidly interrupted current (*e.g.*, sixty shocks per second) is used of such a strength as to cause a *slow* series of ventricular beats, it is found that a diminution in the rate of interruptions (*e.g.*, to six per second), at once leads to the appearance of a much *more rapid* series of ventricular beats. The number of contractions resulting from the application of the slowly interrupted current is much greater than that caused by a rapidly interrupted current; the influence, moreover, of the former is much more lasting than the influence of the latter. A much closer approach to a tetanic condition is induced by a slowly interrupted current than by a rapidly interrupted one. And this statement holds with regard to (1) the ventricle of an intact heart; (2) the isolated quiescent ventricle; and (3) the isolated automatically-contracting ventricle. It holds also with regard to various rates of interruption in the stimulating currents. A current interrupted ten times a second is much more effective than one interrupted fifty times a second. The rule holds, then, that—within certain limits—a slowly interrupted current, whether galvanic or faradic, produces much more striking effects than a rapidly interrupted one, the influence of the latter being much less powerful, and, at the same time, of a less enduring character. It is not necessary when dealing with a slowly interrupted current that the shocks be sent in at any particular phases of the cardiac beat; a series of shocks is simply sent in without regard to the state of the organ at the moment when each shock is sent in.

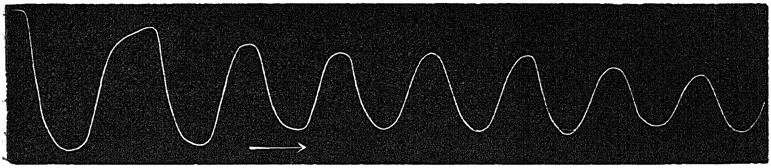
A very striking difference is also evident in the results obtained by the continued application of a weak interrupted current to the ventricle as compared with the *repeated temporary* application of the same current. The latter is commonly able to induce a fairly rapid series of ventricular contractions when the former has no apparent effect at all or merely causes a *very* slow succession of beats.

These remarkable differences in the effects of interrupted currents are manifested not only in the case of the ventricle, but in the auricle and sinus as well—in all the portions of the cardiac muscle.

As a result of the application of *weak* interrupted currents, I have observed no marked effect upon the ventricle of the eel's heart. There



Tracing shows the contractions excited in an isolated quiescent ventricle by stimulating it with a rapidly interrupted current (50 interruptions per second).



Tracing shows the result of stimulating the same ventricle with a slowly interrupted current (5 interruptions per second) of precisely the same strength.

is no evidence whatever of its having the slightest effect in weakening the contractions—a result which has been shown to follow the application of a weak interrupted current to certain cardiac tissues, *e.g.*, the auricle of the tortoise and the ventricle of the frog.

In the case of the intact heart with the normal circulation going on, the passage of a constant current through the apex of the ventricle has been seen to be accompanied by a reversed order of contraction of the parts of the heart, the apex of the ventricle—through which the current is passing—being the portion which initiates the systole of the whole organ.

IV. *Action of the Cardiac Nerves.*

All the nerve impulses affecting the heart appear to pass along the vagus nerve trunks.

Stimulation of the peripheral end of a cut vagus nerve exerts upon the heart an inhibitory influence of a very powerful nature. The inhibitory phase is often followed by a phase during which the heart's rhythm is accelerated. Such an accelerating *after* effect of vagus stimulation is, however, slight in degree and variable in occurrence.

Stimulation of either the right or the left vagus is effective.

When continued stimulation of one vagus fails at length to keep the heart quiescent any longer, stimulation of the other vagus usually causes a further inhibition of the cardiac action.

The latent period of vagus stimulation appears to be short.

The vagus has a very low minimal stimulation; a *very* weak interrupted current such as is insufficient (when applied to the peripheral

end of a cut vagus nerve) to cause contraction of the œsophagus, leads readily to cardiac standstill.

Single induction shocks to the vagus nerve have no distinct effect on the heart's action unless the shocks are very powerful.

When arrested by vagus stimulation, the heart stands still in a state of diastole, and—when the circulation is going on—the whole heart, but more especially the auricle and sinus, become greatly distended with blood, if the inhibition be of any considerable duration. The great veins also become much gorged; they are very capacious, whilst the presence of dilatations upon the jugular veins (jugular sinuses) allows of the accommodation of a large amount of blood.

The effects of cardiac standstill on the circulation can readily be observed by fixing the transparent part of the animal's tail under a microscope, and then inducing inhibition of the heart by stimulation of the vagus nerve, or simply by pressing on the gill—a proceeding which causes reflex cardiac arrest. When the heart stands still, there is a very gradual slowing of the blood-current which at length comes to a state of complete stagnation; this result is not arrived at until the cardiac action has been completely stopped for a period of from one to two minutes. When inhibition has passed away, and the heart's action has recommenced, the blood-flow is very speedily restored; the change is a much more rapid and abrupt one than the change observed when the heart was brought to a standstill; the first recommencing beat causes a distinct movement of blood in the capillaries of the tail.

High intra-ventricular systolic pressure and distension of the heart with blood (caused by clamping the branchial artery) do not obviate the inhibitory effects of vagus stimulation.

The eel's heart presents some striking peculiarities as regards its mode of recommencing action after it has been arrested through the medium of the inhibitory nerves.

It will be remembered that in the case of the frog, toad, rabbit, dog, when the heart's action goes on after an inhibitory standstill, the various parts of the heart recommence action in the fashion in which they normally beat. Thus in the frog, when the cardiac action recommences after a period of inhibition, the heart beats present their usual characters as regards the order of succession; the contraction first affects the sinus, and then passes over the auricles and ventricle successively. In the eel the mode of recommencing is, after *slight* inhibition, similar as a rule to that seen in the other animals mentioned,—the contraction begins in the sinus (ostial part) and passes over the rest of the heart in the normal fashion. But after *profound* inhibition the renewal of cardiac action presents features of an entirely different and very peculiar character. For

when the heart recommences after a prolonged standstill, the order of contraction of the different part is, as a rule, markedly changed, and moreover, the contraction is for the first few beats restricted to certain parts of the organ. The remaining parts of the heart remain quiescent for a time, and then come to participate in its activity; the order of contraction generally remains modified, and it is only after a considerable though variable period that the ordinary succession of events again obtains in the contracting heart.

The part in which spontaneous contractile activity usually reappears after profound inhibition is not the ostial part of the sinus (which ordinarily leads the rhythm of the heart), but the interjugular part of the sinus, which ordinarily contracts second in the series, *i.e.*, in succession to the leading ostial part. The recommencing contraction is commonly—at least after powerful inhibition—confined for one or more beats to the interjugular parts; it often extends, however, at the first beat from the interjugular part to the ventricle. The beating of the interjugular part, soon at least accompanied by responsive ventricle beats, goes on for a short though variable time without the slightest movement being perceptible in the auricle or in the ostial part of the sinus. After a time the contraction originating in the interjugular part of the sinus spreads over the ostial part of the sinus, and soon afterwards over the auricle as well as over the ventricle, so that the whole heart is now in action, though the order of contraction of its various parts is not identical with that normally present. For instead of the systole being initiated by the ostial beat, it is now initiated by the interjugular beat. Soon, however, the ostial beat regains the precedence and the normal order of events is restored.

These phenomena seem to depend (1) on the unequal influence exerted by vagal stimulation on the different parts of the heart; (2) on the high inherent rhythmic power of the interjugular part; and (3) on the existence of the anatomical and physiological connexion between the sinus and ventricle already mentioned. The ventricle is not directly affected at all by vagus stimulation; its excitability and contraction force remain quite unimpaired. The interjugular part and the path between it and the ventricle are less profoundly affected than are the auricle and ostial parts; the former parts recover more readily from the inhibitory influence than do the latter. Early released from inhibitory control, the interjugular part, in virtue of its high rhythmic power, begins to beat, and the contraction is soon propagated to the excitable ventricle. Hence the appearance of the peculiar form of recommencement seen.

In various fishes besides the eel (*e.g.*, carp) I have observed a somewhat similar mode of recommencing action after inhibition. The sinus and ventricle usually contract once or several times before

any movement was perceptible in the auricle. I have not seen a distinct division of the sinus into ostial and interjugular parts in any fish except the eel.

During the inhibitory standstill the condition of the various parts of the heart, as evidenced by the results of direct stimulation, is strikingly different from that observed in the hearts of those animals in which cardiac inhibition has been chiefly studied.

In the frog-heart, for example, a single direct excitation applied to the heart during the inhibited phase produces a single beat; the contraction begins at the stimulated point and extends over the whole organ. And this result is obtained whether auricle or ventricle is stimulated. In the eel's heart the state of matters is more complex. The ventricle resembles the frog-heart in giving a single beat in response to a single excitation; its irritability and contraction force seem to be quite unaffected. The interjugular part, except when the heart is most powerfully inhibited, remains excitable to direct stimulation. And contraction excited in the interjugular part is usually propagated to the ventricle, whilst the auricle remains perfectly quiescent; contraction excited in the ventricle is commonly propagated in a similar fashion to the interjugular part. The ostial part of the sinus is, during strong inhibition, quite inexcitable to direct stimuli of all kinds, and so is the auricle. The result of nerve stimulation has evidently in this instance been the peculiar one of temporarily abolishing the irritability of the muscular tissue. Such a result is quite incompatible with the old conception of vagal action, viz., that the vagus inhibited the motor discharges from the cardiac ganglia, and so brought the muscular tissue into a state of quiescence. If such were the case the muscle ought of course to respond readily to direct stimulation. And this the auricular muscle signally fails to do.

It has been mentioned that during the inhibited state direct stimulation of the ventricle readily causes contraction of that part; the contraction originates at the stimulated point and spreads over the rest of the ventricle. It does not pass over the auricle. The non-participation of the auricle in the contraction cannot be explained by the assumption of a block between the auricle and ventricle preventing the contraction being propagated. For if such were the case, if the auricle remained quiescent merely because the contraction failed to reach it, the auricular muscle ought to respond readily to direct stimulation. But it has been shown that the auricular muscle is quite inexcitable, and that it is in such a state as to be quite incapable of responding to any contraction impulses that might reach it.

Effects of Vagal Stimulation on the Properties of the Muscular Tissue of the different Parts of the Heart.

In the investigation of these effects the cardiac action was registered by means of a simple arrangement of the graphic method. Simultaneous tracings of the auricular and ventricular action were obtained by means of two writing levers suspended in the horizontal position by slender elastic bands; these levers were brought into connexion with the auricle and ventricle respectively by threads. In order to prevent locomotion of the heart it was sometimes necessary to afford a fixed point by holding in a clamp the bands passing into the dorsal aspect of the auricle.

At times a simultaneous record of the contractions of the auricle and ventricle was obtained by a single writing lever: (1) by attaching the threads from the auricle and ventricle to the same lever, or (2) by connecting the lever by the auricle alone (by means of a thread), and then firmly fixing the apex of the ventricle. The auricular and ventricular beats are then recorded on a single tracing.

I shall take each part of the heart in succession, and briefly consider the effects observed in each.

(1.) *Effects of Vagus Stimulation on the Ventricular Muscle.*

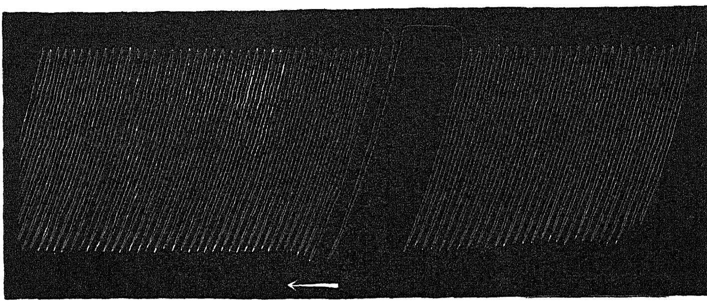
A large number of experiments have clearly shown that the ventricle is not directly affected by stimulation of the vagus nerve.

As has been already mentioned, the automatic rhythmic power of the ventricle is low, and the ordinary rate of action of this part in the intact heart results not from the spontaneous rhythm of the ventricle itself, but in consequence of the rapid action of the sinus and auricle, from which parts the contraction is propagated to the ventricle. The ventricular contraction is started off by the other parts of the heart; its inherent rhythmic power remains latent. It is obvious then that an arrest of the action of the other parts of the heart would at once lead to a suspension of the ventricular activity. The ventricle, deprived of the impulses which ordinarily start off its contraction, must necessarily stand still until such time as the starting off impulses come again into play, or until its latent automatic rhythm begins to manifest itself—an event which, in ordinary circumstances, would not occur for a very considerable length of time. So that with regard to the suspension of the ventricular rhythm caused by vagus stimulation, the removal of the normally efficient cause of that rhythm is the reason of the ventricular standstill, *i.e.*, the ventricle stands still because the other parts of the heart have stopped action.

The excitability of the ventricular muscle during inhibition of the

heart seems (from a comparison of the results obtained by direct stimulation in the normal and inhibited states) to be unchanged. A contraction can be elicited by a direct stimulus applied during the inhibitory standstill quite as readily as in the normal condition of the heart.

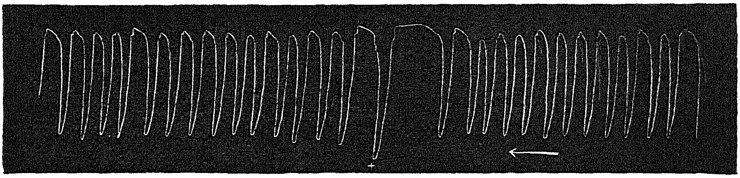
Upon the contraction force of the ventricular muscle, stimulation of the vagus nerve seems to be entirely without effect.



Ventricle tracing showing inhibition resulting from stimulation of vagus nerve. The recommencing beats are seen to be large.

When a heart which is beating at a fairly rapid rate is slowed by very gentle vagal stimulation, the ventricular beats are larger proportionately to the slowing—up to a certain maximum. The increase in the force of the beats is dependent on the slowing; it occurs when the heart is slowed by means other than by vagal influence, *e.g.*, by the application of cold to the sinus. It is the converse of what occurs during rapid action of the heart, such as may result from the application of heat to the sinus, or may follow vagal inhibition as an after effect. During rapid action of the heart the ventricular beats become smaller in proportion to the increased rates, until at length, if the acceleration is excessive, the recording lever writes a mere wavy line on the smoked paper.

When the heart is arrested by vagus stimulation, and the ventricle is made to contract by a direct stimulus, the force of the beat so elicited is, even during the most powerful inhibition, quite as large as the ordinary beats occurring spontaneously before the standstill. Indeed an artificial beat elicited during inhibition is generally larger than the spontaneous beats occurring before inhibition, just as the beats occurring during a very slow cardiac rhythm are larger than those seen during a quicker rhythm. A rapid artificial rhythm induced in the ventricle during inhibition of the heart shows a diminution in the size of the rapidly recurring beats, corresponding to what is seen in a rapid natural rhythm.



Ventricular tracing showing an inhibitory standstill—a result of vagus stimulation. The beat marked + was elicited by direct stimulation of the ventricle during the period of inhibition.

(2.) *Effects of Vagal Stimulation on the Auricle.*

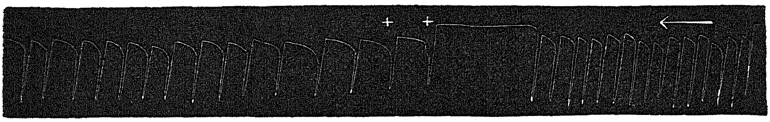
The auricle, though possessing an inherent rhythmic power of its own, has this independent rhythm masked and rendered latent (like the lower rhythmic power of the ventricle) by the more rapid rhythmical lead of the sinus. The auricle then is, in consequence of the rapid action of the sinus, made to beat at a considerably quicker rate than it would otherwise do in virtue of its own inherent rhythm.

When the vagus is strongly stimulated, and the whole heart brought to a standstill, the auricle becomes relaxed, and a marked diminution of its ordinary tone is evident. It stands still not merely in the way the ventricle does because of the arrest of the dominant part which normally starts off its contraction, but because of a profound influence directly exerted on its muscular tissue. Its inherent rhythmic power is markedly depressed, for vagal stimulation can keep the auricle in a motionless state much longer than would be required for the exhibition of its independent rhythmic power. If the auricle merely stopped in consequence of the stoppage of the sinus, and if the inherent auricular rhythmic power was intact, an independent action would very soon be exhibited by the auricular tissue. It is obvious that this inherent automatic rhythmic property is held in abeyance by vagal stimulation.

Moreover, there occurs in the auricle a striking depression of the excitability of the muscular tissue as tested by direct stimulation. The auricular excitability is, during strong inhibition, annulled for the time being. Such a loss of excitability does not depend on over-distension of the auricle with blood during the cardiac standstill; it occurs in the bloodless auricle as well. As the inhibitory phase is passing away direct stimulation of the auricle leads to localised contraction in the stimulated area. These contractions seem to be localised in consequence of the great depression of excitability which still pervades the auricular tissue, and prevents the contraction from spreading over the whole auricle as it would do in the normal state. Very weak stimulation of the vagus nerve causes a distinct depres-

sion of the auricular excitability without any absolute suspension of that excitability. Very weak vagal stimulation is often able to cause the auricle to beat more slowly than the rest of the heart, or to stop action for a time while the other parts of the organ go on beating.

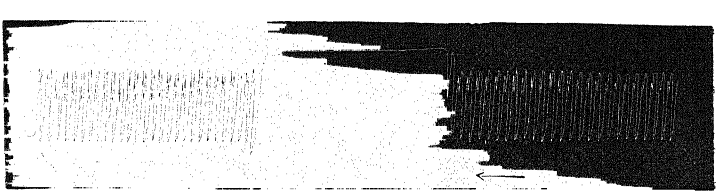
At high temperatures (29—33° C., &c.) the auricular excitability to direct stimulation does not seem to be depressed during cardiac standstill brought about by vagus stimulation.



Tracing of auricle showing vagal inhibition. The beats marked + were elicited by direct stimulation (single induction shocks) applied to the auricle before its action had recommenced. It will be seen that the auricular beats continue to be of diminished size for some time after the spontaneous action has recommenced.

Vagal stimulation is also able to depress very markedly the contraction force of the auricular muscle. Any beats that can be elicited by strong direct stimulation (when the inhibitory phase is passing away) are much diminished in size, and the beats with which the auricular action recommences are as a rule very small ones; they gradually increase till they regain the normal size. The depressing effect on the contraction force accompanying vagal inhibition occurs both in the intact heart and in the bloodless auricle; it is not due to over-distension during the inhibitory period.

The conduction power of the auricle is, during inhibition, temporarily abolished.



Tracing showing the contractions of both auricle and ventricle. The large beats are those of the ventricle; the smaller ones between the large beats are those of the auricle. The effects of vagal inhibition are shown. The recommencing auricular beats are much diminished in size; the recommencing ventricular beats are large.

These primary depressing effects of vagal stimulation on the auricular tissue are by far the most constant and powerful ones; the secondary accelerating effects are slight and variable. The increase

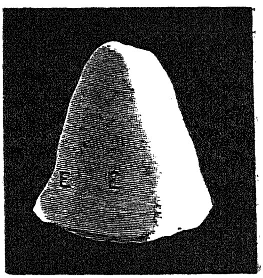
in the rate of the auricular action, which is at times seen as a secondary effect of vagal stimulation, does not depend on the auricle itself, the inherent auricular rhythm being latent; the acceleration depends on the increased rate of action of the sinus which leads the cardiac beat.

I shall here mention some results obtained by the application of electricity to the auricular tissue.

Single induction shocks (as well as mechanical or thermal stimuli) applied to the normal auricle during its diastole cause a single contraction, accompanied as in the ventricle by a bulging of the auricular wall at the point of stimulation.

The passage of a weak interrupted current produces some peculiar effects in the auricle. For when an interrupted current of suitable strength is sent through any part of the auricle, that part immediately stands still, becoming distended with blood, while the rest of the auricle goes on beating as before. By gradually increasing the strength of the current a wider and wider area can be inhibited, until at length the whole auricle becomes arrested, while the sinus and ventricle may still go on beating. Any escape of the current to the sinus will, however, cause a stoppage of the whole heart.

When local inhibition is brought about in the auricle in the way mentioned, there is a marked change in the excitability of the inhibited area, as well as a suspension of its rhythmic activity. Its excitability to direct stimulation is greatly depressed or temporarily annulled; the application of a direct stimulus commonly fails to induce any contraction. The inhibited tissue seems to be in a state similar to that into which the whole auricle is thrown by stimulation of the vagus nerve. The action of the inhibited area is not usually resumed for some little time after the discontinuing of the inhibitory current.



When a projecting corner of the auricle is dealt with, and the electrodes are applied at the points indicated by the letters E and E, only part of the projecting piece of auricle is, as a rule, thrown out of action. The shaded part becomes arrested, while the remainder goes on contracting in unison with the rest of the auricle.

Poisoning with curare (a minimal dose) prevents the occurrence of local inhibition in the auricle as a result of the passage of a weak interrupted current. And when an isolated piece of auricle exhibiting an *independent* rhythm is subjected to the influence of a weak interrupted current, its behaviour differs markedly from that of the same piece of tissue whilst it formed part of an intact auricle, for in the case of an *automatically contracting* isolated piece of auricle the application of a weak interrupted current produces *no* inhibitory effect. When the current is strengthened a depression of the contraction *force* is induced, but the rhythm remains unchecked, unless, perhaps, by *very strong* currents. The rhythm may even be accelerated, and when *very strong* currents cause an apparent standstill, it is doubtful whether this standstill is due to a true inhibition of rhythm.

(3.) *Effects of Vagus Stimulation on the Sinus.*

A. *On the Interjugular Part.*

During the inhibitory standstill of the heart the interjugular part is directly affected by nerve influence; it does not merely stop action because the leading parts have stopped. During vagus stimulation there is a marked depression of the inherent rhythmic tendency. The interjugular part can, by vagus stimulation, be kept quiescent for a very much longer time than would be required for the manifestation of the independent rhythm of the part; there is evidently an inhibition of the inherent rhythmic property. This depression of rhythmic power seems to pass off from the interjugular part earlier than it does from the neighbouring ostial parts.

As a result of vagal stimulation the excitability of the interjugular part (as well as its inherent rhythmic power) is depressed. During moderate excitation of the vagus nerve the interjugular part responds less readily to a direct stimulus, and during powerful inhibition the interjugular part fails to contract at all on the application of a direct excitation. Moreover, when recommencing action begins after a prolonged inhibitory standstill, the contraction is often seen to begin at the dorsal aspect of the interjugular part, and to be for a time limited to a small area of the tissue; at each succeeding beat the contraction extends, and soon involves the whole interjugular part. Such facts indicate the occurrence during inhibition of a marked depression of the excitability of the part.

As a secondary effect of vagus stimulation, there is sometimes a distinct increase in the rhythmic power, and probably also in the excitability of the interjugular part.

The passage of an interrupted current through the interjugular part very readily brings about an arrest of the whole heart. A very weak current is sufficient for this purpose; frequently a current too

weak to be perceived on the tongue causes prolonged cardiac stand-still.

The passage of a constant current through the interjugular part appears to cause an acceleration in the rhythm.

B. *Effects of Vagus Stimulation on the Ostial Parts of the Sinus.*

When either the right or left vagus nerve is stimulated the rhythmic power of both the right and left ostial parts is completely suspended for a time. The excitability to direct stimulation also is temporarily annulled; no contraction is caused by the application of even a powerful direct excitation.

As a secondary effect of vagus stimulation there often occurs a marked heightening in the rhythmic power of the ostial parts, and a consequent acceleration of the rate of the heart's action. Probably there is also in some instances an increase in the excitability of the ostial parts.

Weak or strong interrupted currents applied to the ostial part cause an immediate arrest of the heart's action, with the usual features of the inhibited state. Such a result is obviated by the administration of curare.

III. "On the Structure and Development of the Skull in the Mammalia. Part III. Insectivora." By W. K. PARKER, F.R.S. Received January 15, 1885.

(Abstract.)

Although this paper is confessedly only a fraction of what is necessary to be done in this polymorphic order, it shows at least how difficult a group it is to handle. For the Insectivora are set in the midst of the other Mammalia—low and high. They might be called the Biological stepping-stones from the Metatheria to the Eutheria.

One thing can be done, even now, with our present fragmentary knowledge of the structure and development of the Insectivorous types—we can assure ourselves that these types are immediately above the Marsupials, that they have the Bats (Chiroptera) obliquely above them, that their nearest relations must be sought for amongst extinct Eocene forms, and that, lowly as they are, and arrested and often dwarfed to the uttermost (so that nature could not safely go further in that direction), they are rich in prophetic characters that have come to perfection in larger and nobler types.

I think it will not be denied that in the ascent of the types the Chiroptera are above the Insectivora, and, as it were, a sort of special

"new leader" from that stock, and that the Insectivora are more or less transformed modifications of the Marsupial type. I suspect that the existing Insectivora just yield the zoologist one of his groups of types classed together because he knows not what else to do with them; they are not a proper, clear, special branch or "leader" of the Mammalian life-tree. They form one group under one designation, just as the *poor* of this metropolis form a group; their special mark is simply lowliness; they differ *inter se* almost as much as the whole remainder above them differ. The higher forms, however, because of their elevation, can afford to be subdivided again into order after order. If we could descend and see the transforming and newly transformed Placentalia of the Eocene epoch, then the morphologist and the zoologist would find common ground; the taxonomy of the latter, however, would be as useless as the titles and distinctions of modern society to some undeveloped race of savage men.

The evidently extreme specialization of the existing Monotremes or Prototheria, and their manifest close relationship to the Edentata—a strange lowly group of Eutherians, almost extinct in the old world, and not potent in genera and species in the new—makes it necessary for me in the present stage of my research to leave them until I have mastered both them and the great Marsupial sub-class. Of the latter, however, I can speak already, and as no interpretation of the meaning of the parts seen in a Eutherian skull can be made until they are read in the light of the structure of the *quasi-reptilian* skull of the Marsupial, I shall in this paper compare the two types together, using the lower and older, as a measure of the higher and newer, types of skull.

Anatomists are familiar with the character of the skull in adult Marsupials; to these may be added others that have turned up to me in the study of their development. When these are seen in the light of the types outside and below the Mammalia, then that which is typical in a high Mammal, as such, can be formulated, and the specialization of this great branch of the Vertebrate stock be understood. I will, therefore, here give a list of the more important and striking cranial characters of the Marsupials, promising to bring forward, as early as possible, figures and descriptions of the skull in various stages and in many kinds. But before making this comparison of the characters of the skull in the Marsupials with what is seen in the Insectivora, I will state that in the latter—a mere order—the diversity is fourfold that to be found in the Marsupials (which are worthy to be put not as a mere order, but as a sub-class), for in them, whether they be eastern or western kinds, the uniformity on the whole is very remarkable, as remarkable as the diversity seen in the Insectivora. The problem put to the morphologist, however, is to explain why the characters that distinguish a Marsupial from a high or Eutherian Mammal are for the most part those which the former

possess in common with the Sauropsida. The residuum of proper unique Metatherian characters neither to be found in the higher Mammals, on the one hand, or in the Sauropsida on the other, is but small. Another, and a crucial, difficulty is this—the Sauropsida, which of all others help us most in the interpretation of the Marsupial skull, are not those to be found in low Reptilian, but in the highest Avian, types. Of all birds the Passerinæ are the noblest, and are most marvellously specialized for their own peculiar mode of life, having many accomplishments and high intelligence. Yet it is from this order of birds that I have had most help in this matter, finding in their skulls special structures which closely correspond to what is most remarkable in that of the Marsupials. There are several characters in the *superficial* or *investing* elements of the skull of Marsupials that are unlike what we find in the higher forms of Placental Mammalia, but which linger in the lower.

a. The frontals are very small in proportion to the parietals, and the squamosals are, relatively, especially in the young, inordinately large—as large as the frontals.

b. The lacrymals are not only large, and have generally a facial plate, but they have, as a rule, *two* canals.

c. The palatine plates of the maxillaries and palatine bones form an extremely hollow or dome-like structure, and by the time the creature is full-grown much of their substance has been absorbed, so as to leave larger or smaller fenestræ: thus there is an attempt to return to the schizognathous condition of these parts seen in many Sauropsida.

d. The palatines are often formed of several pieces, very irregular patches of bone, and these irregular centres are largely absorbed, or united with the main parts in the adult skull.

e. The pterygoids are very small, and have their basicranial parts limited on account of the constant separate development of a large meso-pterygoid.

f. The main vomer is often relatively small; there is nearly always a pair of antero-lateral vomers protecting the cartilaginous capsules of Jacobson's organs, and large postero-lateral and often postero-medial vomers. These are very irregular and unsymmetrical, in the young *Cuscus*, especially, in which I find *ten* vomerine bones.

g. The floor of the tympanic cavity ossifies before the cartilage is ripe, but in two sub-equal centres—the annulus and "os-bullæ;" inside the latter a large folded cartilage protects the Eustachian tube, and outside the former the meatus externus is protected by a more or less segmented tube of cartilage, which ends outside in the continuous concha auris.

h. The jugal or malar bone is large, and reaches back so as to lie over the cartilage of the glenoid cavity, thus helping to form the joint.

i. The angular part of the lower jaw is greatly incurved forming a remarkable hollow inside.

In the *Endo-cranium* there are some very curious structures that differ from what we find in the high forms of Mammalia, but which mostly agree with what is seen in the Sauropsida.

a. The nostrils are sub-terminal, and give off large tongue-shaped cartilages to protect Jacobson's organs.

b. The whole nasal labyrinth is small, especially in the young, not more than half as large as in an average placental Mammal, and the cribriform plate is less depressed in front, very limited in size, and is square in form.

c. The orbitosphenoids do not form the presphenoid by meeting together below, but the presphenoid is as independent as the basisphenoid.

d. There is no special optic foramen in the orbitosphenoid, but the optic nerve passes through the common sphenoidal fissure with the orbital nerves and the first branch of the fifth; the second branch, like the third, has its own foramen rotundum, as in Man, and many other Eutheria.

e. The next character is one of the most important; it is this, namely, that the orbitosphenoids are flush with the alisphenoids. The latter, which are extremely large, ossify a tract of the general cartilaginous side-wall of the embryonic skull—the highly developed chondro-cranium—and not a *free flap* of cartilage, merely continuous with the basal bar, as in the Eutheria. For in these latter the more bulky brain pushes out the lower part of the side-wall of the skull, leaving for some time a band of cartilage, which runs free from the alisphenoid, passing from the orbitosphenoid up to the super-occipital. In front, the orbitosphenoid is confluent with the ethmoid, so that but for the breach in the wall made by the alisphenoid, there would be, even in placental Mammals, a chondro-cranium very similar to that of the Skate. This breach does not take place anywhere among the vertebrate types until we get above the Marsupials. The other character just mentioned, namely, the absence of a special optic foramen, is of similar import; there is no such a foramen in the "*Amniota*" until we are among the placental Mammalia.

f. The alisphenoid helps to form the drum cavity by developing behind its small external pterygoid process, a shell-like growth, similar to the "anterior tympanic recess" of Carinate birds. Thus, as the squamosal is a labyrinth of air cavities opening into the upper part of the drum cavity, these and the tympanic recess in the alisphenoid greatly enlarge the space for air. Indeed, not only those parts, but the mastoid region of the auditory capsule, and the sides and top of the occipital arch, all become pneumatic, *as in Crocodiles and Birds.*

g. The internal carotids pierce the basisphenoid submesially, that part of the basis cranii is not perforated in the middle, and the clinoid processes and cavity for the pituitary body are but little developed.

h. The limited, sub-erect, and flattish cribriform plates, and the small frontals, are the necessary correlates of a small brain cavity and brain. The *occipital plane* corresponds with those parts, being sub-erect; it forms but little more than a right angle with the general basi-cranial axis.

i. In the Marsupials as well as in the Monotremes we see the ossicula auditûs, in making, so to speak. In all the sub-divisions of the class, Monotremes or Prototheria, Marsupials or Metatheria, and Placentals or Eutheria, the lower jaw is broken up, the longer part becoming the persistent mandible, the shorter hind part the *malleus*, whilst the starved and modified quadrate becomes the *incus*. For a long while in the growing Marsupial the malleus is manifestly a compound bone; it is an "articulare," with an internal and posterior angular process, as in the Fowl. On it the "angulare" can be seen, and sometimes, as in the half-grown Koala (*Phascolarctos*), a supra-angulare too. The working mandible, attached to a new pier on the jugal and squamosal, is composed of a sort of morphological mixture of a large *inferior labal cartilage*, a dentary bone, with coronoid and splenial *regions*, and the greater part of Meckel's cartilage—*the true primary ramus*.

j. The topmost segment of the next arch (pharyngo-hyal) is often a "columella," and not a *stapes*. In the early young and embryo of the Marsupials it is V-shaped; its greater front fork enlarging above to form the inverted base of the columella or stapes, and then dwindling into a rod; the lesser hind fork becoming, after a time, detached and then ossified, to form the interhyal. In fishes the uppermost element of a branchial arch (the hyoid is a branchial arch) often forks; in the Sturgeon these become two separate pieces, as in this particular case of the embryo Marsupial. There is not much to remark upon in the rest of the hyoid arch, the functional suspensory part.

For comparison with the Insectivora the *existing Marsupials* do not yield me all the archaic characters I want. For the existing low Eutheria are of course the descendants of Metatheria that were much more generalised and archaic than any now existing; these latter during the Tertiary Period must have undergone, on their own low platform, many adaptive changes that would make them look very strange beside the Marsupials of the Secondary epoch, if these latter could be restored for comparison with them.

The best type of Insectivore for general comparison is the Hedgehog (*Erinaceus europæus*), as it shows the least suppression of parts, and the best development of that which is diagnostic, so to speak, of the

order. In it the great investing bones of the skull are similar to those of the Marsupial, but the nasal and squamosals are smaller, and the frontals are larger. In the hard palate there is a considerable relapse, as in Marsupials; certain tracts of bone being absorbed, but it has no mesopterygoids, and only *five* vomers, yet the anterolateral pair are well developed. Moreover the tympanic region has only one annulus, the outer bone; there is no separate os-bullæ. Instead of the latter there is a crescentic shell of bone, which grows from the basisphenoid, greatly increasing the size of the tympanic cavity. In the endoskeleton in front of the tympanic cavity there is a remarkable ridge of bone growing outwards from the alisphenoid. That ridge is the remnant of the alisphenoidal tympanic wing of the Marsupial, and the shell of bone growing from the basisphenoid is the same morphological element as the separate os-bullæ, but it has lost its independence. The higher Mammalian type is fully reached in the thorough freedom of the alisphenoid from the general cranial wall. This character, indeed, is intensified into the special diagnostic of an Insectivore, for it lies almost wholly outside the orbitosphenoid. Here the sphenoidal fissure, which in this case lets out the second branch of the fifth, but not the optic nerve—that nerve having its own foramen in the orbito-sphenoid—is not a mere gap, but a *side passage*, or a sort of sphenoidal corridor, right and left. In these things the Hedgehog is higher than the Marsupial, but in some others it is lower, or more archaic. These latter characters, which suggest an uprise from a more general type than the existing Metatheria, are—

1st. The development of solid hyaline cartilage in the pterygoid region, a remnant of the pterygo-quadrate of the Ichthyopsida.

2ndly. The presence of a persistent pituitary hole, which is connected with a curiously specialized structure, only seen in typical Insectivores, namely, a hollowing out of the basis cranii beneath the pituitary region.

3rdly. A third archaic character, not seen in the existing Marsupials, is the huge relative size, long persistence, and separate distal ossification of Meckel's cartilage, so that in the embryo Hedgehog, and even in the nestling, the primary lower jaw is as large as in fishes generally, scarcely excepting the Selachians.

The ossicula auditùs are typically Eutherian; we have lost the imperforate stapes or columella, the interhyal is very small or absent, and the malleus and incus are much like what we find in the higher Mammals, generally. The pneumaticity of the skull is much reduced: the olfactory region is almost double the relative size of that of a Marsupial. In the head of another family of the Insectivores, namely, the Mole (*Talpa europæa*) there is much that is in accord with what is found in its distant relation the Hedgehog, but in it there are evident signs of degradation and of relapse into what is Marsupial in

character. The nasal labyrinth is relatively immense, and the skull walls below, laterally, and behind, are as exquisitely pneumatic as in the Flying Marsupial (*Petaurus*), the Bird, or the Crocodile. The swollen basis cranii, all air galleries within, is so excavated that the hinder sphenoid, both base and wings, largely helps the flat single tympanic to form the drum cavity. The pituitary hole does not exist, but there is a considerable pterygoid cartilage. The ossicula *in the adult* are normal, but a curious special character is seen in the ossification, in the young, three parts grown, of the sheath of the stapedial artery, which for a time holds the stapes in its place. It is, however, absorbed afterwards, but remains in the related genus *Myogale*. In nearly half-grown young Moles the malleus is quite like that of the Marsupials; it is an evident "articulare," with copious wild growths of bone, sub-distinct, which answer to the "angulare" and "supra-angulare" of a Reptile or Bird. This malleus in its articular part has two endosteal and one ectosteal bony centre.

Meckel's cartilage, long continuous with the Malleus, is nearly as massive as in the Hedgehog, and has a more distinct separate ossification in its sub-distal part, a long, independent, but temporary *hypobranchial bone*.

The Mole shows a most remarkable development of the endocranium, which, twenty years ago, suggested to me that its skull retained unmistakable Monotrematous characters. In large young of the *Echidna* and *Ornithorhynchus* the solidity of the chondrocranium is immense, like that of a *Chimæroid* Selachian, and the investing bones are thin and splintery. I have not made out the mode of ossification of the inner skull in those types, but in *spirit*, if not in the *letter*, the Mole agrees with them, that is, in the great development and independence of the inner skull. The opisthotic bone ossifies the normal petro-mastoid region, whilst the prootic bony centre begins in its right place on the front edge of the cartilaginous capsule, and then runs away from it into the wall of the skull. Thus there is a large bony tract in the temporal region between the squamosal and the large interparietal, which is not one of the ordinary ectocranial bones, but an endo-cranial bony tract overshadowing and yet imitating the true temporal bone or squamosal. This bone is represented by three separate centres in osseous fishes, namely, the prootic, pterotic, and sphenotic, whilst their true auditory region is partly ossified by the epiotic and opisthotic; the epiotic is only sub-distinct in the Mole. If I am asked why I dive so far down for my illustrations, instead of being satisfied with what Reptiles and Birds would show me, my answer is that these are often of no use for comparison, as they are as thoroughly specialized for their own mode of life as the Mammalia, generally, and are as completely, and often more completely, transformed from the original archaic type or types. Thus the Mole,

like most of the Edentata lately described by me, suggests as the root stock of the Eutheria, generally, not Marsupials (Metatheria), as we know them, but Prototherian forms in which, in ages long past, the existing Monotremes and Marsupials had a common origin. The Shrew (*Sorex vulgaris*) represents another family of the Insectivores, the Soricidæ. It combines the characters of the Mole and Hedgehog with peculiarities of its own that are manifestly due to dwarfing; many things are suppressed, as if there was not room in so small a skull for their development. The pituitary hole reappears, and the pterygoid cartilage, but the tympanic wings of the alisphenoid and of the basi-sphenoid are gone. The malleus does not show itself so unmistakably Marsupial, and Meckel's cartilage is slenderer. The sheathing alisphenoids are well seen, the squamosal is extremely small, low down, and devoid of a jugal process; the jugal bone is suppressed. The prootic wing is present, as in the Mole.

So much for the British representatives of these families of the Insectivora—the Erinaceidæ, Talpidæ, and Soricidæ. The Mascarene Insectivora are so evidently related to each other, as to suggest, at once, a common origin; these are the Centetidæ, the largest of which is the Tenrec (*Centetes ecaudatus*); the other genera treated of in this paper are *Ericulus*, *Hemicentetes*, and *Microgale*.

These are almost typical Insectivora, but they agree with the Shrews in having the jugal bone suppressed; they are also more Marsupial than our native kinds. In these types the normal characters of the skull of an Insectivore are combined with a remarkable Marsupial tympanic wing to the alisphenoid, but the os-bullæ is not free, it is merely an outgrowth of bone from the basisphenoid. The pituitary hole is present and in the large species the curious basicranial excavation; the optic foramina also and the sphenoidal side passages are remarkably developed. As in the genus *Phalangista* among the Marsupials, and *Sorex* and *Talpa* among the British Insectivora, the antero-lateral vomers are evidently suppressed, or have a very temporary independent existence; the postero-lateral vomers are rather small, as in the Hedgehog. In the embryo the main vomer is *relatively* as large as in the embryo Whale, and is curiously cellular or spongy. In nestlings this one primary azygous centre has broken up into three; one, the largest, above, and two lesser below, sheathing, it as it sheathes, the base of the nasal septum. Now this multiplication of the vomers, proper, is thoroughly Marsupial. It is unique, as far as I know, in the mode of its subdivision into secondary bony centres. In the African (Continental) family the Elephant or Jumping Shrews (Macroscelidæ), as illustrated by the largest forms *Petrodromus* and *Rhynchocyon*, we have a curious mixture of Marsupial or Metatherian and Eutherian characters, so that they are aberrant as Insectivores; the Marsupial characters are most remarkable:—

These are (1) the absence of an optic foramen in the embryo; (2) the alisphenoids scarcely overlapping the orbitosphenoids; (3) the tympanic wings of the alisphenoids are well marked, hollow shells in the embryo; (4) large antero-lateral vomers, and postero-lateral vomers, as large as in average Marsupials, and, as in many of them, meeting and uniting at the mid-line; (5) a large distinct "os-bul'æ," which makes a tympanic cavity as large as, and much like that of, *Petaurus* or *Phascolarctos.* On the high Eutherian side we have, in the embryo, frontals as large as the parietals, and strangest of all Mammalian specialization, a long *proboscis*, composed of thirty double rings of cartilage, a structure quite similar to the proboscis of an Elephant. The mesopterygoids are suppressed, but the pituitary hole is present.

I now come to a type for which no place can be found in our systems of Zoology, but for which the late Professor Peters, in despair, lodged with the Insectivora; I refer to the Flying Cat (*Galeopithecus*). This genus forms a Family by itself, and yet has only two species; it should form an Order, as the Hyrax does.

These two species of Flying Mammals are full of *remnants* of what is old, and *rudiments* of the *new*. I put them between the most archaic (Marsupials) and some of the most curiously modified Eutheria, the Frugivorous Bats, and survey them from these two widely separate standpoints; but they possess that which neither Phalanger nor Bat will account for or explain.

With a flat, outspread, foliaceous skull, as completely ankylosed as that of any bird, and as thoroughly pneumatic in its post-orbital region, we have one of the largest and most perfect *hard palates;* with the upper incisors partly suppressed, the lower incisors well developed and utterly unique, and the premolars and molars strong for grinding. The cheek bones and the squamosals are large and thoroughly Marsupial, so are the small external pterygoid processes and internal pterygoid bones, and the very large mesopterygoids. I find no antero-lateral vomers, but Jacobson's organs and their protecting cartilages are *twice as long* as in any types yet examined, and the postero-lateral vomers are almost as well developed as in Marsupials, whilst the main vomer is very large. The sphenoid bones are typically Eutherian, but the basisphenoid has beneath it, *as in Lizards*, a small "parasphenoid;" this I find only in *G. phillippensis*, and as yet in no other Mammal. As in the Marsupials, the jugal or malar helps to form the glenoid cavity, and the squamosal is as large as in *Cuscus*, the lowest of the *Eastern Marsupials.* The single flat tympanic bone, with its ossified and compressed meatus, is very remarkable; but this part of the skull corresponds neither with the Marsupials nor the Insectivores, and this is true also of several other of its characters.

Those things in which it agrees with the Marsupials are not the same as in the Hedgehog; it differs from both Insectivores and Marsupials in its own peculiar way, and in some things is more archaic than either. This type appears to me to be a waif from a large group of forms that were beginning to be transformed out of the Metatheria into the Flying Eutheria (Chiroptera), certain of which, this living type among the rest, being arrested at the general level (or platform) of the Insectivora; they are equal to, rather than members of, the order Insectivora. The last type to be mentioned is the Tupaia, an Eastern form, rather high in position, yet combining characters for the first time seen in the Mammalia, namely, a perfect orbital ring, with old Metatherian structures, such as the large os-bullæ, the small external and internal pterygoids, and a somewhat absorbed hard palate. The last three kinds, *Rhynchocyon*, *Galeopithecus*, and *Tupaia*, all show a curious mixture of that which looks upwards to the highest types, and of that which has been retained from the lower and more archaic forms of the Mammalian class.

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February 5, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

I. "The Relation of Bacteria to Asiatic Cholera." By E. KLEIN, M.D., F.R.S., Joint Lecturer on General Anatomy and Physiology at the Medical School of St. Bartholomew's Hospital, London. Received January 28, 1885.

I propose to bring before the Royal Society the results of an inquiry into the etiology of Asiatic cholera, undertaken, at the instance and expense of the Secretary of State for India, by myself, Dr. Gibbes, and Mr. Alfred Lingard while in India. This investigation will be published *in extenso* by the India Office, but permission has been granted to us to bring to the notice of the Society some of the more important points of our inquiry, particularly those regarding the relation of bacteria to Asiatic cholera. I shall supplement them by giving the results of further observations which I have made since my return from India.

As is now well known, Dr. Robert Koch, in an extensive inquiry into the etiology of cholera in Egypt, Calcutta, and in France, 1883–84, undertaken by him, Drs. Gaffky and Fisher, at the instance of the German Government, has arrived at certain conclusions, which briefly stated are these:

1. In all persons suffering from Asiatic cholera there occur in the rice-water stools during the acute stage of the disease certain well-characterised bacteria, which, on account of their curved shape, Koch called "comma bacilli."

2. These comma bacilli are mobile rods, of small size, of about the same thickness as tubercle bacilli, but only of half their length; they are always more or less curved, sometimes as much as to form half a circle; they vary in length according to the state of growth; they occur either singly or in couples, in the latter case arranged like an S.

3. The comma bacilli occur in great numbers in the mucus flakes as well as in the fluid of the choleraic evacuations. They occur in the lower part of the ileum of persons dead in the acute stage almost to

the exclusion of other bacteria, and in such great numbers that the lower part of the ileum may be considered to contain almost "a pure cultivation of comma-bacilli."

4. The mucous membrane of the ileum, particularly that of the lower part, around and in the lymphatic glands located here—the solitary and Peyer's lymph-glands—exhibits in typical and rapidly fatal cases characteristic alterations: loosening and detachment of the epithelium of the surface and of that lining the glands of Lieberkühn; swelling of the mucous membrane and congestion of its blood-vessels, particularly at the peripheral portions of the lymph glands. These alterations are due to the presence, growth, and multiplication of the comma bacilli in these tissues, and the disease cholera is caused by the production on the part of these comma bacilli, and by the absorption on the part of the system of a special chemical ferment.

This state of the presence of the comma bacilli in the tissue is best pronounced in the lower part of ileum; higher up it is more limited, and gradually diminishes, and finally disappears in the upper part of the small intestine.

5. The blood and other tissues are free of any organisms.

6. The comma bacilli grow well outside the body at the ordinary temperature of the room, but better still at higher temperatures up to 38° or 40° C. They divide transversely; after division the two offsprings may remain joined end to end with shape of an S, and by further division they may grow into a spiral-like or wavy form. They grow well in the mucus flakes taken from the intestine, and placed on linen kept in a moist cell; they grow well on potato, in broth, in Agar-Agar jelly, in solid nourishing gelatine mixture (gelatine, peptone, and beef extract). In this latter substance they exhibit a peculiar and definite mode of growth not seen by Koch on any other bacteria. The comma bacilli require for their growth an alkaline medium; they are killed by acid, by drying, and various antiseptic media.

7. On account of their constant occurrence in the intestines of patients suffering from Asiatic cholera, on account of their absence in all other diseases of the intestine, and on account of their peculiar mode of growth in nourishing gelatine, Koch vindicates for these comma bacilli not only an important diagnostic value, but also considers them as the true cause of cholera.

8. Since his return to Germany, Koch has convinced himself of the correctness of the observations of Nicati and Rietsch, who maintain that cholera can be produced in dogs and guinea-pigs by injecting directly into the small intestine of these animals the comma bacilli taken either directly from the choleraic evacuations, or from artificial cultivations.

Our investigations enable us to say this:

1. Koch's statement as to the almost constant occurrence of comma bacilli in the rice-water stools of cholera patients is correct; the comma bacilli vary greatly in numbers in different stools and in different cases, in some being exceedingly scarce, in others numerous.

2. These comma bacilli vary greatly in length, some being twice and three times as long as others, some well curved as much as to form half a circle, others showing only just a slight bend. The name comma bacillus is inappropriate, as in reality they are vibrios.

3. The comma bacilli occur in the mucus flakes of the rice-water stools as well as in those taken from the ileum of a person dead of cholera. The sooner after death the examination is made, the fewer comma bacilli are found in the mucus flakes; in several typical rapidly fatal cases the mucus flakes taken from the ileum and examined soon after death (from between fourteen minutes and an hour or an hour and a half) contained the comma bacilli only very sparingly indeed, and not to the exclusion of other bacteria. Our investigations do not bear out Koch's statement as to the lower part of the ileum being in acute typical cases of cholera almost "a pure cultivation of comma bacilli." In not one of the many post-mortem examinations of typical acute cases have we found such a state.

4. The mucous membrane of the ileum of typical rapidly fatal cases, if examined soon after death, does not contain in any part any trace of a comma bacillus or any other bacteria, not even in the superficial loosened epithelium.

If the post-mortem examination is sufficiently delayed, comma bacilli and other bacteria may be found penetrating into the spaces of the mucous membrane.

The theory of Koch's as to the comma bacilli present in the mucous membrane secreting a chemical poison inducing the disease cannot, therefore, be correct.

5. Neither the blood nor any other tissue contains comma bacilli or any other micro-organisms of known character.

6. The behaviour of the comma bacilli in artificial media is not such as to justify their being considered as specific. They grow well in alkaline and neutral media, are not killed by acids, and their mode of growth in gelatine mixtures, however peculiar, is not more peculiar than that of other putrefactive bacteria; they show marked differences when grown in different media, but not more so than the ordinary putrefactive bacteria when compared in their growth with one another. The manner in which the choleraic comma bacilli grow in gelatine is identical with that shown by the comma bacilli of the mouth of healthy persons (Lewis) in that same medium.

7. Koch overlooked that "comma bacilli" occur in other intestinal

diseases, in the mouths of healthy persons, and as shown recently, even in some common articles of food.

8. The experiments performed by Koch and others on animals do not in the least prove that the comma bacilli are capable of producing cholera or any other disease. The results obtained by them are much easier explained in a manner opposed to that given by Koch and others.

9. There is direct evidence to show that the water contaminated with choleraic evacuations, and containing, of course, the comma bacilli, when used for domestic purposes, including drinking, by a large number of persons, did not produce cholera.

10. The mucus flakes taken from the small intestine of a typical rapidly fatal case of cholera contain numerous mucus corpuscles filled with peculiar minute straight bacilli; in this state they are found when the examination is made very soon after death; soon, however, the mucus corpuscles swell up and disintegrate, and then their bacilli become free.

The small bacilli are never missed in the mucus flakes. They are only one-third or one-fourth the length of the comma bacilli, and about half their thickness. They are non-mobile; they grow well in Agar-Agar jelly, but show in their mode of growth no peculiarity by which they could be considered as specific. When grown on the free surface of the nourishing material they form spores.

11. These small bacilli are not present in the blood, in the mucous membrane of the intestine, or in any other tissue.

12. Experiments made with these small bacilli on animals produced no result.

13. Since my return to London I have ascertained that the comma bacilli of cholera show two distinct modes of division, one the known one of transverse division, and a second one of division in length. When growing in Agar-Agar jelly at the ordinary temperature of the room, after some days the bacilli swell up owing to the appearance in their protoplasm of one or more vacuoles; as these vacuoles increase, so the comma bacilli become gradually changed first into plano-convex, then into oblong bi-convex, and ultimately into circular corpuscles. The longer the original comma bacillus, the larger the final circle. These circular organisms are mobile just as the comma bacilli, and by disintegration of the protoplasm at two opposite points two perfect more or less semicircular comma bacilli are formed. Growing the comma bacilli in Agar-Agar jelly kept at higher temperatures (30–40° C.), they multiply by transverse division only, but transferring these to Agar-Agar jelly and keeping this at the ordinary temperature of the room, they again gradually change into circular organisms, which, by division in the diameter of the circle, form two new comma bacilli.

February 12, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on the Condensation of Gases at the Surface of Glass." (Preliminary.) By J. T. Bottomley, M.A., F.R.S.E. Communicated by Professor Sir William Thomson, F.R.S. Received January 27, 1885.

It is well known to those who have endeavoured to obtain, in glass vessels, the very perfect vacuums first sought after and obtained by Crookes, and producible by the mercurial pumps, that the operation is much assisted by heating the glass vessels to be exhausted, and even the tubes of the pump, to a high temperature. The difficulty of removing the film of air and moisture adhering to glass tubes is also well known to makers of barometers and thermometers.

When the Sprengel pump is used for producing a vacuum, and when a tolerably good vacuum has been produced, so that the barometric gauge indicates a presence of one millimetre or half a millimetre of mercury, the drops of mercury falling in the tube of the Sprengel give rise to a loud metallic hammering sound; and they fall with such unbroken sharpness that those who use this form of pump are often troubled by the "fall-tubes" splitting longitudinally through a length of several inches—a phenomenon in itself very remarkable, considering the strength of the tubes and the smallness of the mercurial drops. If, while this hammering is going on, the glass vessel which is being exhausted and the leading tubes of the Sprengel pump be heated by passing the flame of a spirit-lamp or of a Bunsen burner over them, the hammering immediately ceases, and on looking closely at the fall-tubes it is seen that they are carrying down air which the heat has liberated from the glass walls of the apparatus. The ordinary barometer gauge is scarcely sensitive enough to show an increase of pressure, but the McLeod gauge readily shows it.

There is another well known phenomenon connected with the condensation of gases and vapours on the surface of glass, viz., the condensation of a watery film over the glass of electric apparatus, in virtue of which, at temperatures considerably above the dew point,

the glass supports are not insulators of electricity. This film of moisture is removed by exposing the glass stems to heat, or to an artificially dried atmosphere. Some years ago, at the wish of Sir William Thomson, I endeavoured to weigh this film of moisture, but was absolutely unsuccessful. The film must be of extreme tenuity. Professor Quincke has, however, made important researches on the "distance of capillary action," and on some of the properties of these very thin films. His results are given in two papers, "Poggendorff's Annalen," 108, p. 326, 1859; and "Wiedemann's Annalen," vol. ii, 1877, p. 145. He finds their thickness to be comparable with 5×10^{-5} cm.

With the view of measuring the quantity of gas condensed upon a given surface of glass, I caused to be prepared in August last a large quantity of fine glass thread. Some of this was of flint glass rod or cane, which was softened in the blowpipe flame, and drawn out on to a wheel. The remainder was of flint glass tubes, drawn out in a similar way. The spun glass was carefully parcelled up in paper, and put aside till I should be ready to use it.

On January 3rd I put a quantity of the non-tubular glass fibre into a glass tube 2 cm. in diameter and 12 cm. long, and attached it by a glass sealing to a five-fall Gimingham Sprengel pump. The pump, which was in excellent order, was then worked rapidly till I had produced a very good vacuum, which by the McLeod gauge gave me an indication of 0·3 **M*** pressure. The pump was then left for about an hour, and at the end of that time, passing one more bottle full of mercury through the pump, I ascertained that the vacuum had not sensibly deteriorated, the McLeod gauge giving identically the same reading as before. This exhaustion was performed without the application of any unusual heat to the tube containing the glass fibres. The temperature of the room was about 56° F.

I now raised the mercury to the upper level, and allowed it to flow through the pump, and the drops fell with the well-known loud hammering noise. While this was going on I applied a Bunsen burner to the tube containing the spun glass. In a few seconds the hammering of the mercury ceased, and on applying the test of the McLeod gauge the pressure within the pump was found to have risen largely. I did not, however, obtain a measurement with the gauge corresponding to the maximum pressure of the gas driven off, or to any particular state.

I now proceeded to pump out all the gas I could, working the pump, and heating the tube containing the glass fibres strongly. The heating was carried on from time to time till the tube, which was of German glass, showed signs of softening and of falling in; and the glass fibres were likewise, some of them, slightly softened and bent.

The pump was worked for over an hour, the heating being applied, and the gas, which was easily seen being carried down, was collected

* 1 **M** standing for one-millionth of one atmo.

in a tube made for the purpose, which fitted on over the up-turned ends of the five fall-tubes. At the end of this time the vacuum was again fairly good, though not so good as it was before the heating commenced. The McLeod gauge indicated 1·2 **M.** It was seen that very little more air was being carried down, and I did not wish to push the vacuum farther than, or quite so far as, the vacuum which had been obtained before the liberation by heat of the condensed gas.

The collecting tube was now removed, and the gas obtained was measured and analysed, so far as it was possible to analyse a quantity so small.

The total amount of gas collected was calculated to be, at 15° C. and a pressure of 760 mm., 0·45 of a cubic centimetre. To this a small quantity of strong solution of caustic potash was added, and time was given for absorption. A small quantity of pyrogallic acid was next added, and the further absorption observed. The residue was so small that I could do nothing farther.

The result of the analysis showed 8·24 per cent. of the whole to be carbonic acid gas (absorbable by caustic potash). Of what remained 24·8 per cent. was oxygen (absorbable by pyrogallic acid and caustic potash mixed). The residue 75·2 per cent. was, I presume, mainly if not wholly nitrogen. I ought to remark that my pump was furnished, as is usual, with the phosphoric acid drying tube. The gas, therefore, which I collected was perfectly dry, and I have no way at present of ascertaining how much moisture adheres to the spun glass. In stating the results of the analysis I have made no correction for moisture introduced with the potash solution.

In order to make an estimate of the amount of surface exposed by the spun glass, I measured, with a screw micrometer gauge, the diameters of 200 of the fine glass fibres taken at random. I found them, as I expected from the care with which they had been prepared, fairly uniform, and the average diameter was 7·06 hundredths of a millimetre. Weighing also the 200, and then the whole quantity, I found the whole number of the fibres to be 6370. The average length was 10·25 cm. The surface was thus 1448 sq. cm., or equal to that of a square 38 cm. in the edge.

I am preparing for further experiments on this subject, and hope soon to be able to add to it observations on the amount and on the electric conductivity of the film of moisture condensed upon the surface of glass.

Additional Note. Received February 12.

Since the writing of my former communication on this subject, I have made some further experiments on it, and I beg leave to give an account of the results of one of these experiments.

Having filled a fresh tube with fresh spun glass, I carefully exhausted with the Sprengel pump on January 24th, and the exhaustion was kept up till February 5th, that is, for twelve days. During this time I frequently tested with the McLeod gauge. A very slight increase of pressure was found during that interval; but it was so slight that I am not able to say that it was greater than that which is observed at all times, even with the Sprengel pump in excellent order, when a vacuum is maintained for several days.

On February 5th, I passed three or four bottlesful of mercury through the pump, and had a vacuum of about 0·5 **M** as shown by the McLeod gauge. I then applied heat, and had instantly an abundance of gas given off from the spun glass. This was collected as before, and analysed.

The number of glass fibres was 15,500, giving an estimated surface area of 3527 sq. centims. The amount of gas given off was 0·41 c.c.; which is considerably less in proportion than in my first experiment.

Of this gas it was found that 78·6 per cent. was carbonic acid gas (absorbable by caustic potash). Of the remainder 10·5 per cent. was oxygen (absorbed by pyrogallic acid and potash); while 89·5 per cent. was left unabsorbed; and may be supposed to be mainly nitrogen.

The very large proportion of carbonic acid gas is remarkable, and it is difficult to account for, unless we may suppose that it was taken up by the glass in large quantity during the operations of drawing out the glass into fibres, and enclosing it in the containing tube—operations during which there was, in these preliminary experiments, an abundant supply from the blowpipe flames.

II. "On Underground Temperatures, with Observations on the Conductivity of Rocks, on the Thermal Effects of Saturation and Imbibition, and on a special Source of Heat in Mountain Ranges." By JOSEPH PRESTWICH, M.A., F.R.S., Professor of Geology in the University of Oxford. Received January 24, 1885.

(Abstract.)

The author remarks on the difference of opinion between physicists and geologists respecting the probable thickness of the outer crust of the earth—the former, on the strength of its great rigidity and the absence of tides, contending for a maximum thickness and comparative solidity of the whole mass; while the latter, in general, on the evidence of volcanic action, the crumpling and folding of the strata in mountain ranges, its general flexibility down to the most

recent geological times, and the rate of increase of temperature in descending beneath the surface, contend for a crust of minimum thickness as alone compatible with these phenomena.

The question of underground temperature, which is a subject equally affecting the argument on both sides, had engaged the author's attention in connexion with an inquiry respecting volcanic action, and he was induced to tabulate the results to see how far the usually received rates of increase were affected by various interfering causes—not that most of them had not received due attention, but it was a question whether sufficient allowance had been made for them.

Although Gensanne's first experiments were made in 1740, and others were subsequently made by Daubuisson, Saussure, and Cordier, in coal and other mines, it was not until the construction of deep artesian wells commenced in the second quarter of this century, and Walferdin introduced his overflow thermometer, and precautions were taken against pressure, that the more reliable observations were made and admirably discussed by Arago. The Coal Commission of 1866 collected a mass of important evidence bearing on the question, and in 1867 a Committee of the British Association was appointed to collect further information. Under the able superintendence of Professor Everett, a series of valuable experiments with improved instruments has been made, and full particulars published in the Annual Reports of 1868—1883.

But notwithstanding the precautions taken, and the accuracy of the experiments, they present very wide differences in the thermometric gradient, ranging from under 30 to above 120 feet per degree Fahr. Consequently different writers have adopted different mean values. On the Continent one of 30 metres per degree C. has been commonly adopted, while in this country some writers have taken a mean of 50 feet per degree, and others of 60 feet or more. The object which the author has in view is to see whether it is not possible to eliminate the more doubtful instances, and to bring the probable true normal gradient within narrower limits. In so doing he confines himself solely to the geological side of the inquiry.

In a general list, Table I, he gives all the recorded observations in the order of date. The list embraces observations at 530 stations in 248 localities. The most reliable of these he classifies under three heads, in Tables II, III, and IV.

1. Coal mines.
2. Mines other than coal.
3. Artesian wells and bore-holes.

To which tunnels are added in a supplement.

The author then proceeds to point out that the gradients given in many of the earlier observations were wrong in consequence of neglecting the height of the surface, and from the exact mean annual

temperature of the locality not being known. They also differed amongst themselves from taking different surface temperatures, and starting from different datum levels. To these he endeavours to assign uniform and corrected values.

The essential differences in the results in the several tables depend, however, upon dissimilar geological conditions, which unequally affect the conductivity of the strata, and disturbing causes of different orders. In the mines the latter are—

1. The currents established by ventilation and convection.
2. The circulation of underground waters.
3. Chemical reactions.
4. The working operations.

And in artesian wells—

1. The pressure of the water on the thermometers.
2. Convection currents in the column of water.

In the later experiments pressure has been thoroughly guarded against, but against the subtle influence of the other causes, though long known, it is more difficult to guard.

Coal Mines.—The author then proceeds *seriatim* with each subject, commencing with coal mines. In these he shows that ventilation and convection currents have rendered many of the results unreliable, as he shows to have been the case in the well-known instance of the Dukinfield coal pit. The circulation of air in coal pits varies from 5000 to 150,000 cubic feet per minute, and tables are given to show how this variously affects the temperature of the coal at different distances from the shaft *though on the same level.* As a rule, the deeper the pit the more active is the ventilation, and therefore the more rapid the cooling of the underground strata. In some pits the indraughted air has been known to form ice, not only in the shaft, but icicles in the mine near the shaft.

The cooling effects of ventilation are shown to begin immediately that the faces of the rock and coal are exposed, and as the hotter (and deeper) the pit, and the more gassy the coal, the more active is the ventilation, so these surfaces rapidly undergo a cooling until an equilibrium is established between the normal underground temperature and the temperature of the air in the gallery. Judging by the effects of the diurnal variations on the surface of the ground, it is clear that when there is a difference of 10° to 12° or more between the air in the gallery and the normal temperature of the rock, an exposure even of a few days must tell on the surfaces of both coal and rock to the depth of the 3 to 4 feet—the usual depth of the holes in which the thermometers are placed. The designation of "fresh open faces" is no security, as that may mean a day or a week, or more. The author considers also that so far from the length and permanence of the experiment affording security, he is satisfied

on the contrary that those experiments in which it is stated that the thermometer has been left in the rock for a period of a week, a month, or two years without any change of temperature, affords *primâ facie* evidence of error, inasmuch as it shows that the rock has so far lost heat as to remain in a state of equilibrium with the air at the lower temperature in constant circulation.

Another cause of the loss of heat which requires some notice is the escape of the gas, which exists in the coal either in a highly compressed, or, as the author thinks more probable, in a liquid state. A strong blower of gas has been observed to render the coal sensibly cooler to the touch. In another case whereas the temperature of the coal at the depth of 1269 feet was 74° F., at the greater depth of 1588 feet in a hole with a blower of gas it was only 62°. One witness observed that "the coal gives out heat quicker than the rock." There is generally a difference of 2° or 3° between the two.

On the other hand, the coal and rocks when crushed and in "creeps" acquire a higher temperature owing to the liberation of heat by crushing.

The effects of irregularities of the surface on the underground isotherms, although unimportant in many of our coal-fields, produce very decided results in the observations on the same level in the mines among the hills of South Wales. Sections are given to show how the temperature rises under hills and falls under valleys, showing that it is often essential to know not only the depth of the shaft but the depth beneath the surface at each station where the experiments are made.

The author therefore considers that to assign a value to an observation we should know—1. Height of pit above sea level. 2. The exact mean annual temperature of the place. 3. Depth beneath the surface of each station. 4. Distance of the stations from the shaft. 5. Temperature and columns of air in circulation. 6. Length of exposure of face. 7. Whether or not the coal is gassy. The dip of the strata and the quantity of water are also to be noted.

Very few of the recorded observations come up to this standard, and the author has felt himself obliged to make a very restricted selection of cases on which to establish the probable thermometric gradient for the coal strata. Amongst the best observations are those made at Boldon, North Seaton, South Hetton, Rosebridge, Wakefield, Liége and Mons. These give a mean gradient of 49½ feet for each degree F. The bore-holes at Blythswood, South Balgray, and Creuzot give a mean of 50·8 feet.

Mines other than Coal.—The causes affecting the thermal conditions of these mines are on the whole very different to those which obtain in coal mines. Ventilation affects both, but in very unequal degrees. In mineral mines it is much less active, and the cooling effects are

proportionately less. On the other hand the loss of heat by the underground waters in mineral mines is very important. In some mines in Cornwall, the quantity of water pumped up does not exceed 5 gallons, while in others it amounts to 200 gallons per minute. The Dolcoath mine used to furnish half a million gallons of water in the twenty-four hours, while at the Huel Abraham mine it reached the large quantity of above 2,000,000 gallons daily. The rainfall in Cornwall is about 46 inches annually, and of this about 9 inches pass underground. In the Gwennap district, where 5500 acres were combined for drainage purposes, above 20,000,000 gallons have been discharged in the twenty four hours from a depth of 1200 feet. This water issues at temperatures of from 60° to 68°, or more than 12° above the mean of the climate, showing how large must be the abstraction of heat from the rocks through which the waters percolate.

Hot springs are not uncommon in these mines. They are due to chemical decomposition, and to water rising in the lodes and fissures from greater depths. The decomposition which goes on in the lodes near the surface, and whereby the sulphides of iron and copper are reduced ultimately to the state of peroxides and carbonates of those metals, is a permanent cause of heat, especially apparent in the shallower mines. On the other hand, where the surface waters pass rapidly through the rocks, they lower the temperature, and give too low readings.

While ventilation, therefore, reduces the rock temperature, the water which percolates through the rock, and more especially through the veins and cross-courses, sometimes raise, and at other times lower the temperature of the underground springs. Mr. Were Fox, who for many years made observations on the underground temperature of the Cornish mines, gave the preference to the rocks, while Mr. Henwood, an observer equally experienced and assiduous, considered that the underground springs gave surer results. Both were of course fully alive to all the precautions that in either case it was necessary to take to guard against these causes of interference.

Taking ten of the most reliable of Mr. Henwood's observations at depths of from 800 to 2000 feet, the mean gives a thermometric gradient of 42·4 feet per degree, but Mr. Henwood himself gives us the mean of 134 observations to the depth of 1200 feet, a gradient of 41·5 feet to the experiments in granite, and of 39 feet to those in slate.

Taking the experiments of Mr. Fox in eight mines,* varying in depth from 1100 to 2100 feet, the mean of the experiments made in the rock give a gradient of 43·6 feet per degree. The mean of the two observers give a gradient of 43 feet per degree.

For the foreign mines, in the absence of fuller data, and especially failing in information of the depth of the station beneath the surface,

which in the hilly district of Freiberg and Hungary introduces an element of great uncertainty, it is impossible to arrive at any safe conclusion.

Artesian Wells and Borings.—This class of observations presents results much more uniform, and whereas the mines observations were made, the one in crystalline, and the other in unaltered palæozoic rocks, the wells are, with few exceptions, in the softer and less coherent rocks either of Cretaceous, Jurassic, and Triassic age, which are much more permeable, and, as a rule, much less disturbed.

The causes of interference are mainly reduced to pressure on the instruments and convection currents. The early experiments, where no precautions were taken against these, are, with few exceptions, unreliable, and must be rejected. The larger the bore-hole the greater the risk of convection currents, and Professor Everett has shown that in many cases of deep and large artesian borings, the water which lodges in them is reduced to a nearly uniform temperature throughout the whole depth by the action of these currents. In the deep boring at Sperenberg, before the introduction of plugs to stop these currents, it was found that the temperature near the top of the bore was rendered 4·5° F. too high, and at the bottom at a depth of 3390 feet, 4·6°, if not 6·7°, too high by the currents.

Taking the bore-holes in which the water does not overflow, and where, owing to the precautions against these sources, such as those of Kentish Town, Richmond, Grenelle, Sperenberg, Pregny, and Ostend, we get a mean gradient of 51·9 feet per degree.

Overflowing artesian wells should, if we were sure of all the conditions, give the best and most certain results. Taking those where the volume of water is large, and the observations made by competent observers, as in the case of the wells of Grenelle, Tours, Rochefort, Mondorff, Minden, and others, we obtain a mean of 50·2 feet, or taking the two sets of wells, of 51 feet per degree.

The author, however, points out a source of possible error in those wells, arising from a peculiarity of tubage which requires investigation, and owing to which he thinks the water may suffer a loss of heat in ascending to the surface.

With respect to the extra-European wells, more particulars are required. It may be observed, however, that the wells in the Sahara Desert, which were made by an experienced engineer accustomed to such observations, the mean of eleven overflowing wells, at depths of from 200 to 400 feet, gave 36 feet per degree.

Tunnels.—For the Mont Cenis Tunnel, allowing for the convexity of the surface, Professor Everett estimates the gradient at 79 feet, and for the St. Gothard, 82 feet per degree. But Dr. Stapff found in the granite at the north end of the tunnel a much greater heat and more rapid gradient, for which there seemed no obvious explanation.

Though this axis of the Alps is of late Tertiary date, the author points out that it cannot be due to the protrusion of the granite, as the Swiss geologists have shown that the granite was in its present relative position and solidified before the elevation of this last main axis of the Alps, and he suggests that the higher temperature may be a residue of the heat caused by the intense lateral pressure and crushing of the rocks which accompanied that elevation, for in the crushing of a rigid material such as rock, almost the entire mechanical work reappears as heat.

Conductivity of the Rocks. Effects of Saturation and Imbibition.—Some of the apparent discrepancies in the thermometric gradients are no doubt due to differences in the conductivity of the rocks. Applying the valuable determinations of Professors Herschel and Lebour to the groups of strata characterising the several classes of observations, the following results are obtained:—

	Mean conductivity. k.		Mean resistance. r.
1. Carboniferous strata	·00488	....	275
2. Crystalline and schistoze rocks.	·00546	....	184
3. Triassic and cretaceous strata ..	·00235	....	465

From this it would appear that the conductivity of the rocks associated with the mineral mines is twice as great as that of the artesian wells class. But all the experiments, with the exception of three or four, were made with blocks of dried rocks, and those showed a very remarkable difference; thus, for example, dry New Red Sandstone gave k 0·00250, whereas when wet it was increased to k 0·00600. The author remarks that as all rocks below the level of the sea and that of the river valleys are permanently saturated with water, dry rocks are the exception, and wet rocks the rule in nature, consequently the inequalities of conductivity must tend to disappear. The power of conduction is also greater along the planes of cleavage or lamination than across them, and therefore the dip of the strata must also exercise some influence on the conductivity of different rocks and "massifs." With respect to the foliated and schistose rocks, M. Jannettaz has shown that the axes of the thermic curve along and across the planes of foliation and cleavage, are in the following proportions:—

Gneiss of St. Gothard	1 : 1·50
Schists of Col Voza	1 : 1·80
Cambrian Slates, Belgium	1 : 1·98

This cause will locally affect the rock masses.

Conclusion.—The author deduces from the three classes of observations a general mean thermic gradient of 48 feet per degree Fahr., but

he considers this only an approximation to the true normal gradient, and that the readings of the Coal-mines and Artesian-well experiments are, owing to the causes he enumerates, still too high. He also discusses the question whether or not the gradient changes with the depth. His own reduction of the observations gave no result, but he points out that in all probability the circulation of water arising from the extreme tension of its vapour is stayed at a certain depth; while as it is known experimentally that the conductivity of iron diminishes rapidly as the temperature increases, this may possibly in a different degree apply to rocks. If, therefore, there is any change, these indications would be in favour of a more rapid gradient.

Taking all these conditions into consideration, the author inquires whether a gradient of 45 feet per degree may not be nearer the true normal than even the one of 48 feet obtained by the observations.

III. "On the Connexion between Electric Current and the Electric and Magnetic Inductions in the surrounding Field." By J. H. POYNTING, M.A., late Fellow of Trinity College, Cambridge, Professor of Physics, Mason College. Birmingham. Communicated by Lord RAYLEIGH, M.A., D.C.L., F.R.S. Received January 31, 1885.

(Abstract.)

This paper describes a hypothesis as to the connexion between current in conductors and the transfer of electric and magnetic inductions in the surrounding field. The hypothesis is suggested by the mode of transfer of energy in the electromagnetic field, resulting from Maxwell's equations investigated in a former paper ("Phil. Trans.," vol. 175, pp. 343—361, 1884). It was there shown that according to Maxwell's electromagnetic theory the energy which is dissipated in the circuit is transferred through the medium, always moving perpendicularly to the plane containing the lines of electric and magnetic intensity, and that it comes into the conductor from the surrounding insulator, not flowing along the wire.

Symbolising the nature of the induction by unit tubes drawn in the direction of the induction in the usual way, *i.e.*, so that there is unit quantity of induction over every section of a tube, the electric induction is equal to $K \times$ electric intensity $\div 4\pi$, and the magnetic induction is equal to $\mu \times$ magnetic intensity. The electric induction is the same quantity as Maxwell's "displacement." The hypothesis now made

consists in the supposition that the electric and magnetic inductions are transferred through the medium in a similar manner to the energy, the induction being regarded as propagated sideways rather than along the lines or tubes of induction. The tubes of electric induction move in upon the wire containing a current, and are there broken up or dissolved, while the magnetic tubes, which are ring-shaped, contract upon the wire and finally disappear, the places of the tubes thus lost being supplied by fresh tubes sent out from the seat of electromotive force.

Examining the basis of Maxwell's electromagnetic theory, it is seen to rest on three principles; viz.:—

I. The electric and magnetic energies are distributed through the field in a manner which can be assigned. Dividing the unit tubes into unit cells by level surfaces drawn at unit differences of potential, each electrical cell, according to Maxwell, contains half a unit of energy, and each magnetic cell $\frac{1}{8\pi}$ unit.

II. The line integral of the electric intensity round any closed curve is equal to the rate of decrease of the total magnetic induction through the curve.

III. The line integral of the magnetic intensity round any closed curve, is equal to $4\pi\times$ the current through the curve.

The second and third principles are modified in a manner suggested by the supposed movement of the tubes of induction.

II modified. Whenever electromotive force is produced by a change in the magnetic field or by motion of matter through the field, the E.M.F. per unit length, or the electric intensity, is equal to the number of tubes of magnetic induction, cutting or cut by the unit length per second; the E.M.F. tending to produce induction in the direction in which a right-handed screw would move if turned round from the direction of motion, relatively to the tubes, towards the direction of the magnetic induction.

This will give the result of II above, if the quantity of magnetic induction through a curve only alters by the movement of tubes in or out across the boundary. Reasons for the supposition are given.

III modified. Using the term magnetomotive force, a term suggested by Mr. Bosanquet, to denote the line integral of the magnetic intensity round the axis of a tube of induction, the third principle is as follows:—

Whenever magnetomotive force is produced by change in the electric field, or by motion of matter through the field, the magnetomotive force per unit length is equal to $4\pi\times$ the number of tubes of electric induction cutting or cut by unit length per second, the magnetomotive force tending to produce induction in the direction in which a right-handed screw would move if turned round from the direction of the

electric induction towards the direction of motion of the unit length, relatively to the tubes of induction.

These principles are applied to special cases.

A Straight Wire carrying a Steady Current.

When a current C is in a wire, C induction tubes are supposed to close in upon the wire per second, these being, as it were, broken up or dissolved, their energy appearing finally as heat.

This accounts for the constancy of current at all parts of the wire in the steady state, in so far as it reduces this constancy to a particular case of the law, according to which there is the same total induction over all sections of a tube.

Also, since C tubes are broken up in the wire per second, and the field is steady, C tubes must move inwards through any curve encircling the wire, and this will, by the third principle as modified, give a line integral of magnetic intensity round the curve equal to $4\pi C$.

Since the electric intensity is not produced by static charges, we must suppose it due to the motion of magnetic tubes. If E is the electric intensity, E magnetic tubes must cut any unit length parallel to the axis of the wire per second moving inwards.

On the supposition that the tubes bring their energy in with them, it is shown that the electric and magnetic tubes each account for half the energy, so that we may suppose that the energy crossing any surface is equally divided between the two kinds.

On the supposition that the tubes can be identified throughout their motion in the insulating medium, their velocities are calculated.

Discharge of a Condenser through a Fine Wire.

When the terminals of a charged condenser consisting of two parallel plates are connected by a wire, the energy which was before the discharge chiefly between the two plates, now appears as heat in the wire. The electric induction tubes are supposed to move outwards from the space between the plates, keeping their ends upon the plates or wires. They finally converge upon the wire and are there broken up. The hypothesis is in accordance with the doctrine of closed currents. For the total result is equivalent to the addition of so many closed induction tubes to the circuit, the induction running the same way relatively to the circuit throughout. When the condenser is discharged by imperfect insulation of the dielectric, we may represent the process still as a closed current, the two parts of which are the loss of induction and its dissipation, but this is artificial, and it is more natural to look upon the process as a decay of induction without movement of fresh induction tubes inwards, and therefore without the formation of magnetic induction. This case is discussed at some length, as we can here realize what goes on at the source of

energy, and the results suggest that a similar action occurs at the source of energy or seat of E.M.F. in other cases.

A Circuit containing a Voltaic Cell.

The chemical theory of the cell is adopted. It is first shown that the difference of potential of the two terminals in open circuit is equal to the E.M.F. immediately after closure, because chemical action will go on charging the terminals, *i.e.*, putting out energy into the medium, until any further charge would require more energy in the medium than is supplied by the chemical action necessary, according to Faraday's law of electrolysis.

The level surfaces are discussed, and it is supposed that the potentials being in ascending order, zinc, copper, acid, all of the surfaces pass between the zinc and the acid, some of them bending round and passing between the copper and acid, the rest going between the terminals. When the circuit is closed, tubes of electric induction, running from acid to zinc, are supposed to diverge outwards and close in on the rest of the circuit, there running in the opposite direction, *i.e.*, from copper to zinc. A divergence of negative tubes being magnetically equivalent to a convergence of positive tubes, the magnetic relations of the circuit or the direction of the current will be the same throughout. The tendency to a steady state is discussed. The existence of charges in the circuit is taken to show that the tubes of electric induction do not enter the wire at the same time throughout the whole of their length.

Current produced by Motion of a Conductor in a Magnetic Field.

In this case it is shown that according to the hypothesis we must suppose a divergence of negative tubes from the seat of E.M.F.

The General Equations of the Electromagnetic Field.

The assumption that if we take any closed curve, the number of tubes of magnetic induction passing through it is equal to the excess of the number which have moved in over the number which have moved out through the boundary since the beginning of the formation of the field, suggests a historical mode of describing the state of the field at any moment.

Taking L, M, N as the numbers of magnetic tubes which have cut unit lengths of the axes through a point since the beginning of the system, those being considered positive which tend to produce positive electric intensity, the equations $a = \frac{d\mathrm{M}}{dz} - \frac{d\mathrm{N}}{dy}$, and two others are obtained. From these and the ordinary current equations $4\pi\mu u = \frac{dc}{dy} - \frac{db}{dz}$, and

two others, we obtain $4\pi\mu u = -\nabla^2 L - \frac{d}{dx}\left(\frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz}\right)$, and two others, which can be solved in the same manner as Maxwell's equations, from which they only differ in form, in signs. The quantity $\frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz}$ is, however, not zero, and on forming the equations for electric intensity, it is found to give rise to a triple integral having value at surfaces of contact of dissimilar substances and at charged surfaces.

In the case when there is no material motion the components of electric intensity are—

$$P = -\mu\iiint\frac{du}{dt}\cdot\frac{1}{r}dxdydz - \frac{1}{4\pi}\frac{d}{dx}\iiint\left(\frac{dP}{dx}+\frac{dQ}{dy}+\frac{dR}{dz}\right)\frac{1}{r}dxdydz,$$

and two other equations.

If the system is steady, $\frac{du}{dt} = 0$.

On putting

$$\frac{dP}{dx}+\frac{dQ}{dy}+\frac{dR}{dz}=4\pi\rho, \text{ and } V=\iiint\frac{\rho}{r}dxdydz,$$

we obtain $P = -\frac{dV}{dx}$, and two other equations.

It is shown that these equations may be obtained without the special hypothesis as to the mode of motion of the magnetic induction by assuming $L=\int Pdt$, $M=\int Qdt$, $N=\int Rdt$.

Equations are also obtained by considering the growth of the electric induction.

February 19, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The Chairman announced that a letter had been received from the Elder Brethren of the Trinity House, in which, after mentioning the experiments on lighthouse illumination which were being carried on at the South Foreland, invitations were offered to any Fellows of the Royal Society who may desire to witness the experiments, and stating further that the Trinity Yacht would be available for observations from the sea.

The following Papers were read:—

I. "On the Structure of Hyaline Cartilage." By GEORGE THIN, M.D. Communicated by Professor SCHÄFER, F.R.S. Received January 29, 1885.

(Abstract.)

The author having previously shown ("Quarterly Journal of Microscopical Science," vol. xvi—New Series), that a laminated appearance is produced in hyaline cartilage by imbibition of nitrate of silver, now shows that the same lamellated structure may be observed in sections of fresh cartilage stained by Bismarck brown, and that the lamellæ, as brought under observation by both methods, have approximately the same thickness and general arrangement.

It seems certain that the matrix of hyaline cartilage is formed by a series of lamellæ superposed over each other, their general arrangement being parallel to the free surface of the cartilage.

Adjoining the zone of ossification, the lamellæ are seen to be very distinctly separated from each other, and at this part their continuity is interrupted by the large spaces which correspond to the position of the cells.

II. "Note on a preliminary Comparison between the Dates of Cyclonic Storms in Great Britain and those of Magnetic Disturbances at the Kew Observatory." By BALFOUR STEWART, M.A., LL.D., F.R.S., and WM. LANT CARPENTER, B.A., B.Sc. Received February 11, 1885.

We took the dates of thirty storms from Mr. Scott's paper entitled "The Equinoctial Gales; do they occur in the British Isles?" in the "Quarterly Journal of the Meteorological Society" for October, 1884, and by the kindness of Mr. Whipple, of the Kew Observatory, were enabled to make the comparison mentioned above.

Out of these thirty storms, in twenty-three cases there is a distinct magnetic disturbance, for the most part preceding the storm by somewhat more than a day. We do not, however, imagine that we have thus proved the fact of such a connexion, but think the results we have attained sufficient to justify us in pursuing the subject.

February 26, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "On a remarkable Phenomenon of Crystalline Reflection." By G. G. STOKES, M.A., Sec. R.S., Lucasian Professor of Mathematics in the University of Cambridge. Received February 25, 1885.

Introduction.

In a letter to me, dated March 29, 1854, the late Dr. W. Bird Herapath enclosed for me some iridescent crystals of chlorate of potash, which he thought were worth my examination. He noticed the intense brilliancy of the colour of the reflected light, the change of tint with the angle of incidence, and the apparent absence of polarisation in the colour seen by reflection.

The crystals were thin and fragile, and rather small. I did not see how the colour was produced, but I took for granted that it must be

by some internal reflection, or possibly oblique refraction, at the surfaces of the crystalline plates that the light was polarised and analysed, being modified between polarisation and analysation by passage across the crystalline plate, the normal to which I supposed must be sufficiently near to one of the optic axes to allow colours to be shown, which would require no great proximity, as the plates were very thin. To make out precisely how the colours were produced seemed to promise a very troublesome investigation on account of the thinness and smallness of the crystals: and supposing that the issue of the investigation would be merely to show in what precise way the phenomenon was brought about by the operation of well-known causes, I did not feel disposed to engage in it, and so the matter dropped.

But more than a year ago Professor E. J. Mills, F.R.S., was so good as to send me a fine collection of splendidly coloured crystals of the salt of considerable size, several of the plates having an area of a square inch or more, and all of them being thick enough to handle without difficulty. In the course of his letter mentioning the despatch of the crystals, Professor Mills writes: "They (the coloured crystals) are, I am told, very pure chemically, containing at most 0·1 per cent. foreign matter. They are rarely observed—one or two perhaps now and then, in a large crystallisation. . . . . I have several times noticed that small potassic chlorate crystals, when rapidly forming from a strong solution, show what I suppose to be interference colours; but the fully formed crystals do not show them."

Some time later I was put into communication with Mr. Stanford, of the North British Chemical Works, Glasgow, from which establishment the crystals sent me by Professor Mills had come. Mr. Stanford obligingly sent me a further supply of these interesting crystals, and was so kind as to offer to try any experiment that I might suggest as to their formation.

I am informed that at the recent Health Exhibition a stand was exhibited from the chemical works at Widnes, showing a fine collection of brilliantly coloured crystals of chlorate of potash. I did not see it. It would seem that the existence of these coloured crystals is pretty generally known, but I have not seen mention of them in any scientific journal, nor, so far as I know, has the subject been investigated.

On viewing through a direct-vision spectroscope the colours of the crystals which I had just received from Professor Mills, the first glance at the spectrum showed me that there must be something very strange and unusual about the phenomenon, and determined me to endeavour to make out the cause of the production of these colours. The result of my examination is described in the present paper.

SECTION I.—*Preliminary Physical Examination.*

1. It will be necessary to premise that chlorate of potash belongs to the oblique system of crystallisation. The fundamental form may be taken as an oblique prism on a rhombic base, the plane bisecting the obtuse dihedral angle of the prism being the plane of symmetry. Rammelsberg denotes the sides of the prism by P, and the base by C, and gives for the inclinations of the faces PP=104° 22′ and CP=105° 35′. The face C, which is perpendicular to the plane of symmetry, is so placed as to bring three obtuse plane angles together at two opposite corners of the parallelopiped. The salt usually forms flat rhombic or hexagonal plates parallel to the C plane, the edges of the rhombus being parallel to the intersections of the P faces by the C plane, and the hexagons being formed from the rhombic plates by truncating the acute angles by faces parallel to the intersection of the C plane by the plane of symmetry.

The plane angles of the rhombic plates, calculated from the numbers given by Rammelsberg, are 100° 56′ and 79° 4′, while the hexagonal plates present end-angles of 100° 56′ and four side-angles of 129°.32′. These angles are sufficiently different to allow in most cases the principal plane of a plate, or even of a fragment of a plate, to be determined at once by inspection. But in any case of doubt it may readily be found without breaking the crystal by examining it in polarised light. There are good cleavages parallel to the two P planes and to the C plane. The crystals are very commonly twinned, the twin plane being C.

2. If one of the brilliantly coloured crystals be examined by reflection, and turned round in its own plane, without altering the angle of incidence, the colour disappears twice in a complete revolution. The vanishing positions are those in which the plane of incidence is the plane of symmetry. The colour is perhaps most vivid in a perpendicular plane; but for a very considerable change of azimuth from the perpendicular plane there is little variation in the intensity of the colour. There is no perceptible change of tint, but on approaching the plane of symmetry the colour gets more and more drowned in the white light reflected from the surface.

3. If instead of altering the azimuth of the plane of incidence a plane be chosen which gives vivid colour, and the angle of incidence be altered, the colour changes very materially. If we begin with a small angle the colour begins to appear while the angle of incidence is still quite moderate. What the initial colour is, varies from one crystal to another. As we increase the angle of incidence the colour becomes vivid, at the same time changing, and as we continue to increase the angle the change of colour goes on. The change is always in the order of increasing refrangibility; for example, from

red through green to blue. Not unfrequently, however, the initial tint may be green or blue, and on approaching a grazing incidence we may get red or even yellow mixed with the blue, as if a second order of colours were commencing.

4. The colours are not in any way due to absorption; the transmitted light is strictly complementary to the reflected, and whatever is missing in the reflected is found in the transmitted. As in the case of Newton's rings, the reflected tints are much more vivid than the transmitted, though, as will presently appear, for a very different reason.

5. As Dr. Herapath remarked to me long ago, the coloured light is not polarised. It is produced indifferently whether the incident light be common light or light polarised in any plane, and is seen whether the reflected light be viewed directly or through a Nicol's prism turned in any way. The only difference appears to be that if the incident light be polarised, or the reflected light analysed, so as to furnish or retain light polarised perpendicularly to the plane of incidence, the white light reflected from the surface, which to a certain extent masks the coloured light, is more or less got rid of.

6. The character of the spectrum of the reflected light is most remarkable, and was wholly unexpected. A direct-vision hand spectroscope was used in the observations, and the crystal was generally examined in a direction roughly perpendicular to the plane of symmetry; but it is shown well through a wide range of azimuth of the plane of incidence. No two crystals, we may say, are alike as to the spectrum which they show, but there are certain features common to all. The remarkable feature is that there is a pretty narrow band, or it may be a limited portion of the spectrum, but still in general of no great extent, where the light suffers total or all but total reflection. As the angle of incidence is increased, these bands move rapidly in the direction of increasing refrangibility, at the same time increasing in width. The character of the spectrum gradually changes as the angle of incidence is increased; for example, a single band may divide into two or three bands.

The bands are most sharply defined at a moderate angle of incidence. When the angle of incidence is considerably increased, the bands usually get somewhat vague, at least towards the edges.

7. The commonest kind of spectrum, especially in crystals prepared on a small scale, which will be mentioned presently, is one showing only a single bright band; and I will describe at greater length the phenomena presented in this case.

When the angle of incidence is very small, the light reflected from the reflecting surfaces of the crystal shows only a continuous spectrum. As the angle of incidence is increased, while it is still quite moderate a very narrow bright band shows itself in some part

of the spectrum. The particular part varies from one crystal to another; it may be anywhere from the extreme red to the extreme violet. It stands out by its greatly superior brightness on the general ground of the continuous spectrum, and when it is fully formed the reflection over the greater part of it appears to be total. The appearance recalls that of a bright band such as the green band seen when a calcium salt, or the orange band seen when a strontium salt, is put into a Bunsen flame. The bright band is frequently accompanied right and left by maxima and minima of illumination, forming bands of altogether subordinate importance as regards their illumination. Sometimes these seem to be absent, and I cannot say whether they are an essential feature of the phenomenon, which sometimes fail to be seen because the structure on which the bands depend is not quite regularly formed, or whether, on the other hand, they are something depending on a different cause.

Disregarding these altogether subordinate bands, and taking account of the mean illumination, it seems as if the brightness of the spectrum for a little way right and left of the bright band were somewhat less than that at a greater distance.

When the main band occurs at either of the faint ends of the spectrum, it is visible, by its superior brightness, in a region which, as regards the continuous spectrum, is too faint to be seen, and thus it appears separated from the continuous spectrum by a dark interval.

When the angle of incidence is increased, the band moves in the direction of increasing refrangibility, and at the same time increases rapidly in breadth. The increase of breadth is far too rapid to be accounted for merely as the result of a different law of separation of the colours, which in a diffraction spectrum would be separated approximately according to the squared reciprocal of the wave-length, while in bands depending on direct interference the phase of illumination would change according to the wave-length.

8. The transmitted light being complementary to the incident, we have a dark band in the transmitted answering to the bright band in the reflected. In those crystals in which the band is best formed, it appears as a narrow black band even in bright light. When the band first appears as we recede from a normal incidence it is extremely narrow, but it rapidly increases in breadth as the angle of incidence is increased.

9. Some of the general features of the phenomenon were prettily shown in the following experiment:—

Choosing a crystal in which the bright band in the reflected light began to appear, as the incidence was increased, on the red side of the line D, so that on continuing to increase the incidence it passed through the place of the line D before it had become of any great

width, I viewed through the crystal a sheet of white paper illuminated by a soda flame. A dark ring was seen on the paper, which was circular, or nearly so, and was interrupted in two places at opposite extremities of a diameter, namely, the places where the ring was cut by the plane of symmetry. The light of the refrangibility of D was so nearly excluded from the greater part of the ring that it appeared nearly black, though slightly bluish, as it was illuminated by the feeble radiation from the flame belonging to refrangibilities other than those of the immediate neighbourhood of D. The ends of the two halves of the ring became feeble as they approached the plane of symmetry. A subordinate comparatively faint ring lay in this crystal immediately outside the main one.

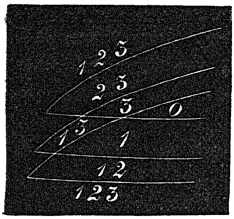
10. Suspecting that the production of colour was in some way connected with twinning, I examined the cleft edge of some of the crystals which happened to have been broken across, and found that the bright reflection given by the exposed surface was interrupted by a line, much finer than a hair, running parallel to the C faces, which could be easily seen with a watchmaker's lens, if not with the naked eye. This line was dark on the illuminated bright surface exposed by cleavage, a surface which I suppose illuminated by a source of light not too large, such as a lamp, or a window at some distance. The plane of incidence being supposed normal to the intersection of the cleavage plane by the C faces, on turning the crystal in a proper direction round a normal to the plane of incidence, the light ceased to be reflected from the cleavage surface, and after turning through a certain angle, the narrow line, which previously had been dark, was seen to glisten, indicating the existence of a reflecting surface, though it was much too narrow to get a reflected image from off it. The direction of rotation required to make the fine line glisten was what it ought to be on the supposition that the fine line was the cleavage face of an extremely narrow twin stratum.

11. On examining the fine line under the microscope, it was found to be of different thicknesses in different crystals, though in those crystals which showed colour it did not vary very greatly. On putting a little lycopodium on the cleavage face interrupted by the fine line, it was seen that in those crystals which showed colour the breadth of the twin stratum varied from a little greater to a little less than the breadth of a spore. The thickness accordingly ranged somewhere about the thousandth of an inch, such being the diameter of the spores. The stratum was visibly thicker in those crystals which showed their bright band in the red than in those which showed it in the blue.

12. That the thin twin stratum was in fact the seat of the colour, admitted of being proved by a very simple experiment. It was sufficient to hold a needle, or the blade of a penknife (I will suppose the

latter), close to or touching the surface of the crystal while it was illuminated by light coming approximately in one direction, suppose from a lamp, or from a window a little way off, and to examine the shadows with a watchmaker's lens. The light reflected from the crystal comes partly from the upper surface, partly from the twin stratum, partly from the under surface, which, however, may be too irregular to give a good reflection. The twin stratum is much too thin to allow of separating the light reflected from its two surfaces in an observation like the present, and it must therefore be spoken of as simply a reflecting surface.

Corresponding to the three reflecting surfaces are three shadows, where the incident light is cut off: (1) from the upper surface, (2) from the twin stratum, (3) from the under surface. Where the body casting the shadow is pretty broad in one part, as the blade of a penknife, the shadows in part overlap. The shadows are arranged as in the figure, where the numerals mark the streams of light reflected from the portions of the field on which they are respectively written, 1 denoting the stream reflected from the upper surface, 2, that reflected from the twin stratum, 3, that reflected from the under surface.



Let the crystal show by reflection, at the incidence at which the observation is made, say a green colour. Then this green colour is seen in the full field 123, though mixed with the white light reflected from the upper surface. The green is a good deal more vivid in the field 23, as the reflection from the upper surface is got rid of. The green is wholly absent from the fields 3, 0, 13, and 1. The field 3, and perhaps also the field 13, may show a little of the complementary red from transmitted light. The distinction between the fields 12 and 123 is not conspicuous, and often cannot be made out. The distinction, so far as it depends on the third shadow, is strongest between 3 and 0, and next to that between 13 and 1. We are not obliged, however, to have recourse to the third shadow, which is often difficult to see; the first two are amply sufficient.

Suppose we take a crystal which is broken at the edge so as to expose a cleavage surface interrupted by the cleavage of the narrow

twin stratum. The stratum usually lies a good deal nearer to one C face than the other. Now when the two faces are turned uppermost alternately, and the distances between the first and second shadows are observed, they are found to be, as nearly as can be estimated, in the same proportion as the distances from the twin stratum to the two faces respectively.

Again, one of the crystals showed the exposed section of the twin stratum slightly inclined to one of the broad faces, which though smooth to the touch did not give a perfect reflection of objects viewed in it. On holding different parts of the blade of a penknife opposite to different parts of this face, the distance between the first and second shadows was found to vary, as nearly as could be guessed, in proportion to the thickness of crystal between the upper face and the twin stratum.

The conclusion was confirmed by observations made with sunlight; but the simple method of shadows is quite as good, and even by itself perfectly satisfactory.

13. Another useful method of observation, not so very simple as the last, is the following. A slit, suppose horizontal, not very narrow, is placed in front of the flame of a lamp at some distance, and an image of the slit is formed by a suitable lens, such as the compound achromatic objective of an opera-glass. The crystal is placed so as to receive in focus the image of the slit, being inclined at a suitable angle, usually in a plane perpendicular to the plane of symmetry. The eye is held in a position to catch the reflected light, and the images formed by the different reflections are viewed through a watch-maker's lens. If the slit be not too broad, the images formed by reflection from the upper surface, from the twin stratum, and from the under surface are seen distinct from each other, so that the light reflected from the twin stratum may be studied apart from that reflected from the upper and under surfaces.

In this mode of observation it can readily be seen, by turning the crystal in its own plane, and noticing the middle image, which is that reflected from the twin stratum, how very small a rotation out of the position in which the plane of incidence had been the plane of symmetry suffices to re-introduce the coloured light, which had vanished in that critical position, which appears to be a position not merely of absence of colour, but of absence of light altogether; at least if there be any it is too feeble to be seen in this mode of observation, though from theoretical considerations we should conclude that there must be a very little reflected light, polarised perpendicularly to the plane of incidence.

14. On allowing a strong solution of chlorate of potash in hot water to crystallise rapidly, in which case excessively thin plates are formed in the bosom of the liquid, I noticed the play of colours by

reflection mentioned by Professor Mills as belonging to the crystals in general at an early stage of their growth. This, however, proved to be quite a different and no doubt a much simpler phenomenon. The difference was shown by the polarisation of the light, and above all by the character of the spectrum of the light so reflected, which resembled ordinary spectra of interference, and did not present the remarkable character of the spectra of the peculiar crystals.

15. When, however, the whole was left to itself for a day or so, among the mass of usually colourless crystals a few were found here and there which showed brilliant colours. These colours were commonly far more brilliant than those of the crystals mentioned in the preceding paragraph, and they showed to perfection the distinctive character of the spectrum of the peculiar crystals. It would have been very troublesome, if possible at all, to examine the twinning of such thin and tender plates as those thus obtained by working on a small scale; but the character of the spectrum, which is perhaps the most remarkable feature of the phenomenon, as well as the dependence of the colour on the orientation, may be examined very well; and thus anyone can study *these* features of the phenomenon, though he may not have access to such fine coloured crystals as those sent me by Professor Mills.

16. A certain amount of disturbance during the early stages of crystallisation, whether from natural currents of convection or from purposely stirring the solution somewhat gently so as not to break the crystals, seems favourable to the production of the peculiar crystals. When the salt crystallised slowly from a quiet solution I did not obtain them.

17. As it is easy in this way, by picking out the peculiar crystals from several crystallisations, to obtain a good number of them, the observer may satisfy himself as to the most usual character of the spectrum. It is best studied at a moderate incidence, as it is sharper than when the incidence is considerable. The spectrum most commonly shows a single intensely bright band, standing out on the general ground of a continuous spectrum of moderate intensity.

A few cases seem worthy of special mention. In one instance two bright bands were seen, one at each faint end of the spectrum, somewhat recalling the flame-spectrum of potassium salts. In another case a red, a green, and a blue band were seen, reminding one of the spectrum of incandescent hydrogen. This crystal in air was nearly colourless at moderate incidences, but showed red at rather high incidences. In another case the crystal was red of intense brilliancy in the mother-liquor, but was colourless when taken out, even at high incidences. Presumably the stratum in this case was so thick that a steeper incidence than could be obtained out of air was required to develop colour.

18. The number of coloured crystals obtained by crystallisations on a small scale, though very small, it is true, compared with the number of colourless ones, was still so much larger than Professor Mills's description of the rarity of the crystals had led me to expect, that I at one time doubted whether the simply twinned crystals which are so very common, if taken at a period of their growth when one component is still very thin, and of suitable thickness, might not possibly show the phenomenon, though the thin twin was in contact on one face only with the brother twin, the other face being in the mother-liquor or in air. The circumstances of reflection and transmission at the first surface of the twin plate must be very different according as it is in contact with the brother crystal, or else with the mother-liquor, or air, or some other fluid; and yet the peculiar spectrum was shown all the same whether the crystal was in air, or immersed in the mother-liquor, or in rock oil. However, to make sure of the matter I took a simply twinned crystal, and ground it at a slight inclination to the C face till the twin plane was partly ground away, thus leaving a very slender twin wedge forming part of the compound crystal, and polished the ground surface. On examining the reflected light with a lens, no colour was seen about the edge of the wedge, where the thickness of the wedge tapered away to nothing; and that, although the bands seen near the edge in polarised light, which was subsequently analysed, showed that had colours been producible in this way as they are by a thin twin stratum, they would not have been too narrow to escape observation.

In another experiment a simply twinned crystal was hollowed out till the twin plane was nearly reached. The hollowing was then continued with the wetted finger, so as to leave a concave smooth surface, the crystal being examined at short intervals in polarised light as the work went on, so as to know when the twin plane was pierced. But though in this case the twin plane formed a secant plane, nearly a tangent plane, to the worked surface, and near the section the twin portion of the crystal must have been very thin for a breadth by no means infinitesimal, as was shown by examination in polarised light, yet no colours were seen by reflection. I conclude therefore that the production of these colours requires the twin stratum to be in contact on *both* its faces with the brother crystal.

19. The fact that a single bright band is what most usually presents itself in the spectrum of the reflected light (though sometimes two or three such bands at regular intervals may be seen) seems to warrant us to regard that as the kind of spectrum belonging to the simplest form of twin stratum, namely, one in which there are just the two twin surfaces near together. The more complicated spectra seem to point to a compound interference, and to be referable to the existence of more than two twin planes very near together; and in

fact in some of the crystals which showed the more complicated spectra, and which were broken across, I was able to make out under the microscope the existence of a system of more than two twin plans, close together. Restricting ourselves to what may be regarded as the normal case, we have then to inquire in what way the existence of two twin planes near together can account for the peculiar character of the spectrum of the reflected or transmitted light.

SECTION II.—*Of the Proximate Cause of the Phenomenon.*

20. Though I am not at present prepared to give a complete explanation of the very curious phenomenon I have described, I have thought it advisable to bring the subject before the Society, that the attention of others may be directed to it.

That the seat of the coloration is in a thin twin stratum, admits I think of no doubt whatsoever. A single twin plane does not show anything of the kind.

For the production of the colour the stratum must be neither too thick nor too thin. Twin strata a good deal thicker than those that show colour are common enough; and among the crystals sent to me I have found some twin strata which were a good deal thinner, in which case the crystal showed no colour.

The more complicated spectra which are frequently observed seem referable to the existence of more than two twin planes in close proximity. There is no reason to think that the explanation of these spectra would involve any new principle not already contained in the explanation of the appearance presented when there are only two twin planes, though the necessary formulæ would doubtless be more complicated.

Corresponding to a wave incident in any direction, in one component of a twin, on the twin plane, there are in general two refracted waves in the second component in planes slightly inclined to each other, and two reflected waves which also have their planes slightly inclined to each other, the angle of inclination, however, being by no means *very* small, as chlorate of potash is strongly double refractin . The planes of polarisation of the two refracted waves are approximately perpendicular to each other, as are also those of the two reflected waves; but on account of the different orientation of the two components of the twin, the planes of polarisation of the two refracted waves are in general altogether different from those of the incident wave and of its fellow, the trace of which on the twin plane would travel with the same velocity. In the plane of symmetry at any incidence, and for a small angle of incidence at any azimuth of the plane of incidence, the directions of the planes of polarisation of the two refracted waves agree accurately or nearly with those of the

incident wave and its fellow. In these cases, therefore, an incident wave would produce hardly more than one refracted wave, namely, that one which nearly agrees with the incident wave in direction of polarisation. In these cases the colours are not produced. It appears, therefore, that their production demands that the incident wave shall be very determinately divided into two refracted waves, accompanied of course by reflected waves.

It seems evident that the thickness of the stratum affects the result through the difference of phase which it entails in the two refracted waves on arriving at the second twin plane. But whereas in the ordinary case of the production of colour by the interposition of a crystalline plate between a polariser and an analyser, we are concerned only with the difference of retardation of the differently polarised pencils which are transmitted across the plate, and not with the absolute retardation, it is possible that in this case we must take into account not only the difference of retardation for the differently polarised pencils which traverse the stratum, but also the absolute retardation; that is, the retardation of the light reflected from the second relatively to that reflected from the first twin plane.

21. I have not up to the present seen my way to going further. It is certainly very extraordinary and paradoxical that light should suffer total or all but total reflection at a transparent stratum of the very same substance, merely differing in orientation, in which the light had been travelling, and that, independently of its polarisation. It can have nothing to do with ordinary total internal reflection, since it is observed at quite moderate incidences, and *only within very narrow limits* of the angle of incidence.

March 5, 1885.

THE TREASURER in the Chair.

The Chairman mentioned that Professor Fowler, President of Corpus Christi College, Oxford, had, on the part of the subscribers to the Henry Smith Memorial, offered to the Society a replica in marble of the bust of the lamented H. Smith; that the Council had accepted the offer, and that the bust could be seen in the anteroom.

The thanks of the Society were given to the donors.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Atkinson, Prof. Edmund, Ph.D.
Baird, Major A. W., R.E.
Bidwell, Shelford, M.A.
Boys, Charles Vernon, A.R.S.M.
Buchanan, John Young, M.A.
Burdett, Henry Charles, F.L.S.
Buzzard, Thomas, M.D.
Carpenter, Philip Herbert, D.Sc.
Cash, J. Theodore, M.D.
Clark, Sir Andrew, Bart., M.D.
Claudet, Frederic.
Common, A. Ainslie, F.R.A.S.
Conroy, Sir John, Bart., M.A.
Creak, Ettrick William, Staff Commander R.N.
Cunningham, Allan Joseph Champneys, Major R.E.
Davis, James William, F.G.S.
Divers, Professor Edward, M.D.
Douglass, Sir James Nicholas.
Ewart, Professor J. Cossar, M.D.
Festing, Edward Robert, Colonel R.E.
Forbes, Professor George, M.A.
Galloway, William.
Goodeve, Professor Thomas Minchin, M.A.
Hicks, Henry, M.D.
Hicks, Professor W. M., M.A.
Japp, F. R., Ph.D.
Jervois, Sir William Francis Drummond, Lieut.-Gen. R.E.
Kennedy, Professor Alexander Blackie, M.I.C.E.
Latham, Peter Wallwork, M.D.
Lewis, Timothy Richards, M.B., Surgeon-Major A.M.D.
Lyster, George Fosbery, M.I.C.E.
MacGillivray, Paul Howard, M.A.
Manson, Patrick, M.D.
Marshall, Prof. A. Milnes, M.D.
Martin, Prof. Henry Newell, D.Sc.
Maw, George, F.L.S.
Meldola, Raphael, F.R.A.S.
Milne, Professor John, F.G.S.
Moxon, Walter, M.D.
Muir, M. M. Pattison, M.A.
Nobel, Alfred.

O'Sullivan, Cornelius.
Perry, Professor John.
Pickard-Cambridge, Rev. Octavius, M.A.
Pogson, Norman, C.I.E.
Pritchard, Urban, M.D.
Pye-Smith, Philip H., M.D.
Ringer, Professor Sydney, M.D.
Rodwell, George F., F.R.A.S.
Sanders, Alfred, F.L.S.
Snelus, George James, F.C.S.
Stevenson, Thomas, M.D.
Tate, Professor Ralph, F.G.S.
Tidy, Professor Charles Meymott, M.B.
Tonge, Morris, M.D.
Tribe, Alfred, F.C.S.
Ulrich, Professor George Henry Frederic, F.G.S.
Unwin, Professor W. Cawthorne, B.Sc.
Vines, Sidney H., D.Sc.
Warington, Robert, F.C.S.
Wharton, William James Lloyd, Captain R.N.

The following Papers were read:—

I. "On the Extraction of Uric Acid Crystals from the Green Gland of *Astacus fluviatilis*." By A. B. GRIFFITHS, Ph.D., F.C.S., Lecturer on Chemistry and Physics, Technical College, Manchester. Communicated by Sir RICHARD OWEN, K.C.B., F.R.S., D.C.L. Received February 6, 1885.

The so-called green glands of the fresh water crayfish lie in the cavity of the head below the front part of the cardiac division of the stomach. The openings of these organs are to be found at the base of each antenna. The organ carefully dissected out of the head of a freshly killed crayfish is seen to consist of two principal parts: an uppermost one which is a transparent and delicate sac-like body filled with a clear fluid, and an underlying portion of a green colour, glandular in appearance, containing granular cells.

In 1848 Professors Will and Gorup-Besanez (see "München Gelehrte Anzeigen," No. 233, 1848) said that this organ probably contained guanin, and from this supposition this green gland has been considered as a secretory organ.

The secretion of this gland is acid to litmus-paper, and on treating the secretions obtained from a large number of green glands with hot dilute sodium hydrate solution, and then adding hydrochloric acid, a slight flaky precipitate was obtained, and on examining these flakes under the microscope they were seen to consist of small crystals in rhombic plates. On treating the secretion with alcohol, these rhombic crystals are deposited; they are soluble in boiling water.

When these crystals are precipitated from the secretion and moistened with dilute nitric acid, alloxanthine ($C_8H_4N_4O_7$) is produced, and on heating this body gently with ammonia, reddish-purple *murexide*, or the "ammonium purpurate" ($C_8H_4(NH_4)N_5O_6$) of Prout

is obtained. This murexide so obtained crystallises in prisms, which by reflected light exhibit a splendid green metallic lustre, and by transmitted light are a deep reddish-purple.

On running in a solution of potassium hydrate upon a microscopic slide containing some of these murexide crystals they were dissolved. From these reactions it is evident that these rhombic crystals are deposits of *uric acid* ($C_5H_4N_4O_3$) from the secretion of the green gland of the crayfish.

On examining the uric acid crystals (deposited from the secretion by means of alcohol) under the microscope, they are seen to be covered more or less with a very thin and superficial coating of some brown colouring matter, probably some pigment.

But, beyond this discovery of uric acid in the secretion of the green gland of *Astacus fluviatilis*, I have found that on treating the secretion with boiling hydrochloric acid a solution was obtained containing in suspension flaky uric acid which was filtered off, and on allowing the filtrate to cool a few crystals (guanin hydrochlorate) separate which are soluble in hot water, and on the addition of ammonia to this hot aqueous solution a precipitate is obtained of *guanin* ($C_5H_5N_5O$), the precipitated guanin being made up of numbers of minute microscopic crystals. On running in warm dilute nitric acid (upon the slide) these crystals disappeared, but were precipitated again on adding a drop of silver nitrate in the form of the nitrate of silver compound ($C_5H_5N_5O,AgNO_3$) of guanin.

I think this investigation proves that this so-called green gland of *Astacus fluviatilis* is a true urinary organ, its secretion containing uric acid and very small traces of the base guanin: the green gland is, therefore, physiologically the kidney of the animal.

II. "On the Atomic Weight of Glucinum (Beryllium). Second Paper." By T. S. HUMPIDGE, Ph.D., B.Sc., Professor of Chemistry in the University College of Wales, Aberystwyth. Communicated by Prof. E. FRANKLAND, F.R.S. Received February 27, 1885.

(Abstract.)

This paper is a continuation of one previously communicated to the Royal Society.* The author has prepared a sample of metallic glucinum, having the composition—

* "Proc. Roy. Soc.," vol. 35, p. 137.

Gl	99·20
GlO	0·70
Fe	0·10
	100·00

and has determined its specific heat at varying temperatures up to 450° with the following results (for pure glucinum):—

c_{100}^{11}	0·4286
c_{145}^{13}	0·4515
c_{193}^{11}	0·4696
c_{240}^{15}	0·4885
c_{312}^{14}	0·5105
c_{360}^{11}	0·5199
c_{417}^{17}	0·5403

These results correspond to the following empirical formula for the true specific heat of the metal at varying temperatures—

$$k_t = k_0 + 2\alpha t + 3\beta t^2,$$

or with numerical values—

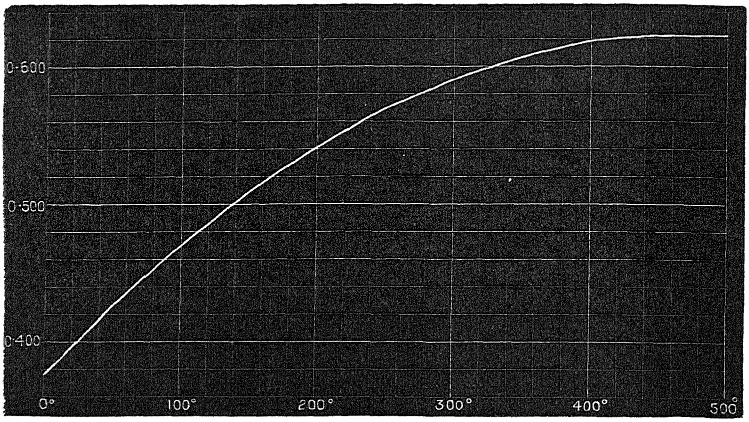
$$k_t = 0·3756 + 0·00106t - 0·00000114t^2,$$

whence the following values for k_t are calculated:—

k_0	0·3756
k_{100}	0·4702
k_{200}	0·5420
k_{300}	0·5910
k_{400}	0·6172
k_{500}	0·6206

In the following curve these values are graphically represented. The curve reaches a maximum at about 470°, and then falls; but whether it represents the specific heat at higher temperatures than 500° is doubtful. The specific heat of glucinum thus rises rapidly up to about 400°, and remains approximately constant between 400° and 500° at 0·62. If this number is multiplied by 9·1 it gives the atomic heat 5·64. Glucinum, therefore, belongs to the same class as carbon, boron, and silicon, which agree with Dulong and Petit's rule at high temperatures only. And the true atomic weight is that required by the periodic law, viz., 9·1 and not 13·6, as was previously deduced from the specific heat between 10° and 100°.

This conclusion is confirmed by the author's determinations of the vapour-densities of glucinum chloride and bromide in a platinum



Curve showing specific heat of Glucinum at varying temperatures.

vessel. The experiments were done in an atmosphere of carbonic acid collected over mercury after Meier and Crafts,* and gave the following results:—

I. *Glucinum Chloride.*

	Substance.	Displaced CO_2.	*t.*	*d.*
E periment I....	26 ·4 mgrms...	7 ·47 c.c.	635°	2 ·733
„ II....	28 ·0 „	.. 7 ·98 „	785°	2 ·714

The theoretical density of $Gl''Cl_2$ is 2·76, and this formula, therefore, represents the molecule of this compound.†

II. *Glucinum Bromide.*

	Substance.	Displaced CO_2.	*t.*	*d.*
Experiment II....	35 ·9 mgrms...	4 ·28 c.c.	608°	6·487
„ III....	61 ·1 „	.. 7 ·53 „	630°	6 ·276
„ IV....	26 ·0 „	.. 3 ·22 „	606°	6 ·245

The density of $Gl''Br_2$ is 5·84, and that of $Gl'''Br_3$ is 8·76. The agreement in this case is not so close as in the case of the chloride, but is sufficiently near to show that the true molecular formula is $Gl''Br_2$, and not $Gl'''Br_3$. Thus, the vapour-density of both compounds necessitates the atomic weight 9·1. The result is a striking argument in favour of the value of deductions drawn from the

* "Berlin Ber.," xiii, 851.

† This result agrees with Nilson and Petterson's experiments ("Berlin Ber.," xvii, 987).

periodic law in regard to the atomic weight of an element, and shows that such deductions will in future form one of the most important factors in fixing a doubtful atomic weight. The author did not appreciate the full value of the periodic law when he wrote his former paper, otherwise he would probably have stated his conclusions less positively.

March 12, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Absorption Spectra of the Alkaloids. Part II." By W. N. Hartley, F.R.S., Professor of Chemistry, Royal College of Science, Dublin. Received March 5, 1885.

(Abstract.)

Having lately completed an examination of a series of aconitines from different sources, kindly sent to me by Dr. Stevenson, Lecturer on Chemistry and Medical Jurisprudence at Guy's Hospital, London, I beg leave to offer the results to the Royal Society.

The list of specimens, which was accompanied by remarks upon them, is the following:—

No. 1. "Exotic aconitine, probably German, rather inert."

No. 2. "A fine specimen of crystallised aconitine, special, prepared by T. Morson and Son, 124, Southampton Row, London."

No. 3. "Aconitine, from Burgoyne, Burbidges, and Co., 16, Coleman Street, London."

No. 4. "Nitrate of aconitine." (This specimen was accidentally destroyed.)

No. 5. "Aconitine of uncertain source."

Of these specimens only two, namely, No. 2 and No. 3, were found to exhibit absorption-bands, and the corresponding curves were drawn from photographs of their spectra.

Morson's fine specimen, the crystals of which were one and even two millimetres in length, was found to absorb the rays at two points, the two absorptions being equally strong. It is noticeable that the most

active aconitines appear to be those with the strongest absorption-bands, and of the commercial samples scarcely two yield the same absorption-spectra. The variations in the curves indicate that not only may there be considerable differences in their composition, but also in their chemical constitution.

Having prepared some of the tertiary bases in a state of great purity, it was considered desirable to examine the spectra transmitted by pyridine, piperidine, quinoline, tetra-hydro-quinoline, and quinoline hydrochloride. It was found that the addition of six atoms of hydrogen to pyridine, and four atoms of hydrogen to quinoline, caused the products to become more diactinic. Hex-hydro-pyridine, otherwise piperidine, shows no absorption-band, as was predicted. Quinoline hydrochloride yields a spectrum differing from that of the base. Substitution products are less diactinic than the simple bases.

As a study of these and similar bodies promises to lead to conclusions apart from such as are of importance in this investigation, and of interest in themselves, a detailed account will be reserved for the present.

The position of the absorption-bands occurring in the spectra transmitted by these tertiary bases may be stated in the following manner:—

			Measurements of absorption-bands in wave-lengths.
Pyridine.		Band between	2700 and 2300
		With less substance, between..	2570 and 2400
Quinoline.			
Two bands	(1) ..	Between	3085 and 3039
		With less substance, between..	3078 and 3039
	(2) ..	Between	3170 and 2600
		With less substance, between..	2980 and 2830
Tetra-hydro-quinoline.			
Two bands	(1) ..	Between	3180 and 2750
		With less substance, between..	3180 and 2870
	(2) ..	Between	2700 and 2300
		With less substance, between..	2650 and 2370
Quinoline hydrochloride.			
Two bands	(1) ..	Between	3180 and 2750
		With less substance, between..	3180 and 2870
	(2) ..	Between	2700 and 2300
		With less substance, between .	2650 and 2370

Substances such as any of the natural alkaloids, which may be derived from dihydroquinoline or tetra-hydroquinoline, by replacement of the hydrogens by other elements or radicals, in such a manner as

to leave the nucleus of the compound unchanged, must be expected to exhibit absorption-bands.

It now appears highly probable—

1. That morphia and some other of the opium bases are derived from pyridine.

2. That strychnine is a derivative of pyridine.

3. That brucine is a derivative of tetra-hydroquinoline, or an addition-product of quinoline of the same character, since there is a remarkable similarity between the absorption curves of the two first-named substances.

I cannot close this paper without acknowledging indebtedness to the great skill and care that my assistant, Mr. W. R. Barnett, has bestowed on these later observations.

II. "Contributions towards the Solution of the Chemical Constitution of Isatin." By H. Kolbe.

[The following pages contain the last experimental researches—unfortunately unfinished—of Hermann Kolbe, upon isatoic acid (Isatosäure) and its transformations. I have endeavoured, in drawing up this paper, to infuse into it the true meaning and spirit of the deceased. This difficult task has been made all the lighter for me from my having had, at the time these investigations were being carried out, frequent conversations with him upon the subject of them, but especially by his own exact notes. Dr. Th. Bellmann and Herr G. Schmidt, who assisted Professor Kolbe in part of the researches herein described, have also given me valuable assistance.

The title of this paper, "Contributions towards the Solution of the Chemical Constitution of Isatin," is from the author himself. He began to write the paper but a few hours before his death. The opening paragraphs (printed in italics) are the last bequest which he has made to us. The concluding ones (also in italics) are taken from remarks made by him which were plainly meant to form a part of this communication.

E. v. MEYER.]

Notwithstanding that so many facts have been collected with the object of definitely settling the constitution of isatin, yet arguments of weight sufficient to support a hypothesis of its rational constitution, or indeed to raise it above the mere level of a hypothesis, are still wanting.

The property of isatin of forming crystalline compounds with the alkaline bisulphites led to the supposition of its belonging to the class of aldehydes or ketones; its property, when in contact with bases, of assimilating the elements of water, and therewith forming isatic acid, from which

stronger acids regenerate isatin, placed it in analogy with oxindol or cumarin, &c., which show a similar behaviour.

These facts alone, as already remarked, were not sufficient upon which to build a solid hypothesis of the constitution of isatin. That this has nevertheless been attempted simply proves how much the necessity was felt of finding for isatin its proper place among those compounds to which it is most nearly related.

Oxidation of Isatin by Chromic Acid. Isatoic Acids.

The first experiments upon the oxidation of isatin, which led to the discovery of the so-called isatoic acid, and also the behaviour of the latter, have been already shortly detailed.* Some of the statements then made as to the supposed composition of the compounds derived from that acid, require correction. Since then isatoic acid and its derivatives have been the subjects of careful investigation.

If the supposition formerly expressed, viz., that isatin is a compound of formyl and nitrogen-benzoyl be correct, then one might expect that it would, by suitable oxidation, be converted into a carboxylic acid, according to the following equation:—

$$\underset{\text{Nitrogen-benzoyl formyl.}}{(CO,C_6H_4N)COH+O} = \underset{\text{Nitrogen-benzoyl carboxylic acid.}}{(CO,C_6H_4N)COOH.}$$

An acid of this atomic composition is in fact formed from isatin; this is the compound termed in the preceding communication Isatoic acid, which name will be retained for it throughout this more complete paper.

The isatin used for its preparation was made in part by myself, by oxidising (natural) indigo by nitric or chromic acid, and partly procured from Schuchardt (in Görlitz) and from Kahlbaum (Berlin).†

A solution of chromic acid in acetic acid has shown itself to be the only suitable oxidising agent for the conversion of isatin into isatoic acid. The following mode of procedure, the result of many trials, has been found to be the best. About 600 grams of glacial acetic acid (the half of the total quantity to be used), are poured over 100 grams of finely powdered isatin placed in a large flask. The chromic acid (200 grams) is brought, in moderate portions at a time (each time about one-sixth of the total quantity), into a funnel loosely stoppered by means of a glass rod, and a part of the remaining 600 grams of acetic acid poured over it; by lifting up the glass rod,

* "Journ. für Prakt. Chem." [2], 30, 84, and 124.

† The fact, perhaps already observed by other chemists, that the isatin got with chromic acid is darker than that got with nitric, is explained by the tenacious adhesion to the first of small quantities of chromic oxide; by dissolving this preparation in hot caustic soda solution and precipitating the filtrate by dilute sulphuric acid, absolutely pure isatin is obtained from it.

the solution is allowed to flow slowly into the mixture of isatin and acetic acid, which thereby becomes heated. As soon as the slightest ebullition is noticeable, the flask must be cooled by ice-water. After the mixture has become quite cold, the process is repeated as above, with frequent shaking, care being taken that the temperature does not rise above 50°, as otherwise the product undergoes partial decomposition.

After each addition of chromic acid, the colour of the liquid, at first red, becomes darker; when about the half of the chromic acid has been added, a brown flaky substance begins to separate out, the quantity of which goes on increasing, and before the whole has been run in, the bottom of the flask is seen to be coated with a dirty grey layer. After the addition of all the chromic acid and acetic acid, the flask, with its dark-brown coloured contents, is allowed, after frequent shaking, to stand in cold water for 12 hours. Should this be neglected, the liquid becomes heated again of itself (the reaction not being yet at an end), and this must be avoided, as it is accompanied by partial decomposition.

At this stage the product contains as yet no isatoic acid. To complete the oxidation, the flask with its muddy contents is now allowed to stand for some hours in water, kept at 40° to 50°; a higher temperature is hurtful. The contents thus undergo a distinct change; the brown flakes vanish, a crystalline substance taking their place, and the liquid finally acquires the deep green colour of acetate of chromium. The product is lastly heated for several hours at about 60°; during this stage isatoic acid separates out in the form of a yellow crystalline powder.

To make this separation as far as possible complete, the whole is poured into 500 c.c. of extremely dilute sulphuric acid; the precipitate thus got, after being washed with cold water with the aid of a filter-pump, and spread out on paper to dry, is almost quite pure isatoic acid. The yield from 100 grams isatin is 80 grams, *i.e.*, 72 per cent. of the calculated amount. It is finally obtained absolutely pure by one crystallisation from boiling acetone. Other solvents are unsuitable for the purification of isatoic acid; water, which, even when warm, dissolves it with difficulty, causes decomposition, with evolution of carbonic acid. Ethyl and methyl alcohols etherify at the boiling temperature a part of the isatoic acid, as may be easily recognised from the agreeable odour which the crystals acquire. Even the minute quantities of methyl alcohol which acetone contains show their presence in this way.

Isatoic acid crystallises from hot acetone (of which about 23 parts at the boiling temperature are required for the solution of 1 part of acid) in hard yellow prisms, which appear to be almost rectangular; it melts at 230°, with decomposition, carbonic acid being given off,

while at the same time a small amount of a substance crystallising in needles sublimes. The yellow colour is characteristic of isatoic acid. It is difficultly soluble in alcohol and glacial acetic acid, pretty difficultly in chloroform and benzol, and very slightly in ether.

The acid (crystallised from acetone) dried over sulphuric acid *in vacuo*, and then at 50°, yielded the following figures on analysis :—

I. 0·4516 gram gave 0·9695 gram CO_2, and 0·1307 gram H_2O = 58·5 per cent. C, and 3·2 per cent. H.

II. 0·3349 gram gave 0·723 gram CO_2, and 0·1307 gram H_2O = 58·9 per cent. C, and 3·3 per cent. H.

0·3336 gram yielded 26·0 c.c. N (=0·03003 gram N) at 16° and 753 mm. pressure = 8·4 per cent. N.

III. 0·284 gram gave 0·612 gram CO_2, and 0·085 gram H_2O = 58·75 per cent. C, and 3·35 per cent. H.

The composition thus found agrees with that of a nitrogen-benzoyl carboxylic acid, $(C_6H_4N,CO)COOH$.

	Calculated.			Found. I.	Found. II.	Found. III.
C_8.....	96	58·9	......	58·5	58·9	58·76
H_5.....	5	3·0	......	3·2	3·3	3·3
N.....	14	8·6	......	—	8·4	—
O_3.....	48	29·5				
	163	100·0				

Adopting my idea of the composition of isatin, the process of its oxidation by chromic acid when the above conditions are observed proceeds, therefore, in this way, that the formyl of the isatin is changed to carboxyl, whereby nitrogen-benzoyl carboxylic acid is formed. (See Equation, p. 194.)

The chemical behaviour of isatoic acid, so far as I have studied it, is in complete harmony with the supposition that it is nitrogen-benzoyl carboxylic acid. Of special importance for the right interpretation of its chemical constitution are its close relations to anthranilic acid, *i.e.*, *o*-amidobenzoic acid. It is readily and completely changed into the latter (or its derivatives) by various reagents, carbonic acid being separated and the elements of one molecule of water assimilated. This transformation of isatoic acid into anthranilic acid is shown by the following equation :—

$$(C_6H_4N,CO)COOH + H_2O = (C_6H_4NH_2)COOH + CO_2.$$

The two atoms of hydrogen of the water combine with the atom of nitrogen, which thereby becomes trivalent to amide; the oxygen atom of the water serves for the oxidation of the carbonyl, carbonic acid

separating; from nitrogen-benzoyl carboxylic acid there thus results amidophenyl carboxylic acid.

Action of Water on Isatoic Acid.

As already mentioned, isatoic acid undergoes decomposition by prolonged boiling with water, carbonic acid being evolved. The dark-coloured powder which separated out on cooling, yielded, after crystallisation from alcohol of 50 per cent., beautiful needles of anthranilic acid (melting point 145°), from which, by solution in hydrochloric acid, the characteristic compound of the latter with anthranilic acid, melting at 191°, was obtained.

Behaviour of Isatoic Acid towards Mineral Acids.

If isatoic acid be gently warmed with ordinary strong hydrochloric acid, decomposition with foaming (evolution of carbonic acid) ensues; the crystalline mass obtained on evaporation is again dissolved in hydrochloric acid, in order to destroy all the isatoic acid. The residue, after solution in water, yields, on cooling, colourless prisms of the hydrochloric-anthranilic acid, containing one molecule of water.

0·392 gram of the compound dried in an exsiccator over sulphuric acid, gave 0·630 gram CO_2, and 0·188 gram H_2O=43·9 per cent. C, and 5·3 per cent. H.

1·152 gram of the same compound, precipitated by nitrate of silver, &c., gave 0·863 gram AgCl, corresponding to 18·5 per cent. Cl.

Calculated for $C_6H_4NH_2,COOH.HCl+H_2O$.				Found.
C_7	84	43·9	......	43·9
H_{10}	10	5·2	......	5·3
N	14	—	......	—
O_3	48	—	......	—
Cl	35·5	18·5	.....	18·5

From this hydrochloric acid compound anthranilic acid itself was precipitated in the usual way by the addition of ammonia and then of glacial acetic acid. From the hot aqueous solution, which has been treated with animal charcoal, it crystallised in colourless prisms an inch long, which, according to the analysis, had the proper composition, and were also recognised as *o*-amidobenzoic acid through its transformation, by means of nitrous acid, into salicylic acid.

0·3475 gram gave 0·159 gram H_2O, and 0·780 gram CO_2=5·1 per cent. H, and 61·2 per cent. C.

Calculated for $C_6H_4NH_2,COOH$, 5·1 per cent. H, and 61·3 per cent. C.

The transformation of the isatoic acid into the hydrochloric-anthranilic acid is complete, as given by the above equation, in which the hydrochloric acid, as taking part in the reaction, must be included.

Tolerably concentrated sulphuric acid acts upon isatoic acid in exactly the same way as hydrochloric acid does; the product is the sulphuric-anthranilic acid, which can in this way be easily got beautifully crystallised in large quantities. The anthranilic acid separated from it gave an analysis corresponding to its composition.

Dilute nitric acid acts upon isatoic acid in a manner analogous to the above two acids, while concentrated turns it into a nitro-derivative which is described below.

Hydrochloric acid gas, acting upon the alcoholic solution of isatoic acid, decomposes it in such a manner that carbonic acid escapes, while the ethyl ether of anthranilic acid is formed. Hydrochloric acid gas was led to saturation, with gradual warming, into alcohol holding finely-powdered isatoic acid in suspension; the resulting clear solution solidified, on cooling, to a mass of pale-yellow needles, which, after washing with alcohol or, better, ether, were analysed. The compound is hydrochloric-anthranilic ether:—

$$C_6H_4NH_2,COOC_2H_5.HCl.$$

0·2330 gram substance gave 0·4602 gram CO_2 (0·1255 gram C), and 0·1298 gram H_2O (0·0144 gram H).

Calculated for the formula $C_9H_{12}NO_2Cl$.		Found.
53·60 p. c. C	......	53·86 p. c. C.
5·95 ,, H	......	6·18 ,, H.

This compound is only slightly soluble in cold alcohol, and not at all in ether. It melts at about 170°, and can be sublimed by cautious heating, but not altogether without decomposition. When brought in contact with water, hydrochloric-anthranilic ether is decomposed into its constituents; the oil which here separated out, and which after washing with water and drying over chloride of calcium, was distilled, is the ethyl ether of anthranilic acid, a strongly refracting liquid boiling at about 260°.

Analysis—

0·3233 gram substance gave 0·7737 gram CO_2 (0·2110 gram C), and 0·1995 gram H_2O (0·0221 gram H).

0·3999 gram substance gave at 15° and 754 mm. pressure 30·2 c.c. N (0·3509 gram N).

Formula.		Calculated.	Found.
C_9	108	65·45 p. c. C	65·26 p. c. C.
H_{11}	11	6·67 „ H	6·85 „ H.
N	14	8·49 „ N	8·77 „ N.
O_2	32	19·39 „ O	
	165	100·00	

Action of Bases upon Isatoic Acid.

Basic substances decompose isatoic acid likewise with evolution of carbonic acid; since this takes place even at a low temperature, it has not been found possible to prepare salts of isatoic acid, as these undergo immediate decomposition into salts of carbonic and of anthranilic acids.*

Isatoic acid, when heated with baryta-water, yielded besides carbonate of baryta the salt of anthranilic acid, which was extracted by saturating the solution with acetic acid and then shaking up with ether; its melting point, as well as that of its hydrochloric acid compound, conclusively proved that it had been formed.

The action of ammonia on isatoic acid is to this extent different, that not the ammonium salt of anthranilic acid, but its amide, which, so far as my knowledge goes, is not yet known, is formed.

Isatoic acid is readily taken up by aqueous ammonia; the solution immediately contains carbonate of ammonia, and after some time solidifies to a mass of yellow crystals, which, after being redissolved in hot water, separate out from the latter in glancing mother-of-pearl plates; the mother-liquor yields, on evaporation, a further quantity. This compound, which is extremely soluble in warm water and in alcohol, melts at 108°, and distils, with slight decomposition, towards 300°. Chloroform is a capital solvent for it, and from this it crystallises in large white plates; it is difficultly soluble in ether and hardly at all in benzol.

Analysis of Anthranilic Amide.

I. 0·346 gram substance, dried over sulphuric acid, gave 0·776 gram CO_2 and 0·195 gram H_2O.

II. 0·155 gram gave 0·350 gram CO_2, and 0·084 gram H_2O.

III. 0·234 gram gave 41 c.c. N at 17° and 759 mm. pressure.

IV. 0·353 gram gave 0·793 gram CO_2, and 0·200 gram H_2O.

From this follows :—

* That, on heating isatoic acid with soda-lime, anilin is set free, is explained by the previous formation of anthranilic acid, whose decomposition product it is.

Calculated for				Found. I.	II.	III.	IV.
C_7....	84	61·76 p. c.	......	61·15	61·58	—	61·27
H_8 ..	8	5·88 „	......	6·27	6·02	—	6·29
N_2....	28	20·58 „	......	—	—	20·31	
O	16						
	136						

A solution of caustic soda converts anthranilic amide slowly and hydrochloric acid very quickly, into anthranilic acid (or its salts).

The same conversion of isatoic acid into this amide is brought about by the action of ammonia gas on the dry (isatoic) acid; at about 60° a vigorous action, with strong foaming, begins, and in a short time the whole is converted into anthranilic amide. The formation of the latter in this, as well as in the preceding case, is seen by the following equation:—

$$\underset{\text{Ammonic isatoate (hypothetical).}}{(C_6H_4N,CO)COONH_4} = \underset{\text{Anthranilic amide.}}{C_6H_4NH_2,CONH_2} + CO_2.$$

Anilin acts upon isatoic acid in an analogous manner to ammonia. A mixture of equivalent molecules of both was warmed to 60°, at which temperature the action, under strong foaming,* began and completed itself. The resulting clear solution solidified on standing for a short time, to a mass of glancing plates; these crystals, after being pressed and washed with a little dilute hydrochloric acid, to free them from any adhering aniline, separated out from solution in benzol, in which they are difficultly soluble, in white colourless needles melting at 130°.

Analysis of this Compound.

I. 0·200 gram of the substance, which had been dried for a long time at 80°, gave 0·105 gram H_2O and 0·541 gram CO_2.

II. 0·140 gram substance gave at 16° and 739 mm. bar. 16·3 c.c. N.

Calculated for $C_6{H_4 \atop NH_2}\Big\}COHNC_6H_5$.				Found. I.	II.
C_{13}....	156	73·58 p. c.	....	73·75	—
H_{12} ...	12	5·65 „	......	5·83	—
N_2....	28	13·20 „	......	—	13·19
O	16				
	212				

* A small quantity of ammonia came off along with the carbonic acid.

According to these figures the compound is *anthranilic anilide*, $C_6H_4NH_2,CONHC_6H_5$, its formation corresponding to that of the amide, and being shown by the following equation:—

$$(C_6H_4N,CO)COOH + \left.\begin{matrix} C_6H_5 \\ H_2 \end{matrix}\right\} N = (C_6H_4NH_2,CO)NHC_6H_5 + CO_2.$$

Anthranilic anilide dissolves pretty readily in water, and very readily in alcohol, ether, chloroform, and acetone; from the alcoholic solution it separates out in colourless prisms. Hydrochloric acid decomposes it gradually into anthranilic acid and aniline.

Action of Concentrated Nitric Acid on Isatoic Acid.—Nitro-Isatoic Acid.

Nitric acid of 1·48 sp. gr. dissolves isatoic acid readily without heating or perceptible evolution of gas, and is thereby coloured red, and gives off ruddy fumes. On standing for several hours the whole solidifies to a homogeneous crystalline mass. After being freed from the mother-liquor by suction through glass-wool, and recrystallised from a mixture of equal parts of absolute alcohol and acetone, the compound is obtained in beautiful mother-of-pearl glancing plates. Its composition gives the formula $C_8H_4N_2O_5$, and it is therefore a nitro-isatoic acid, $\left.\begin{matrix} C_6H_3 \\ NO_2 \\ N \end{matrix}\right\} CO.COOH.$

On the first mother liquor from the crystals being mixed with water, a voluminous precipitate subsided, which, after suction, as before, was also recrystallised from a mixture of alcohol and acetone. The solution deposited, upon cooling, similar crystals to those above, and these were subjected to analysis.

I. 0·260 gram of the acid, after drying over H_2SO_4, gave 0·438 gram CO_2 and 0·052 gram H_2O.

II. 0·132 gram of the same substance gave, at 11° and 760 mm. bar., 15·0 c.c. N.

Calculated for				Found.
C_8	96	46·15 p. c.	....	45·96 p. c.
H_4	4	1·92 „	....	2·22 „
N_2	28	13·46 „	....	13·55 „
O_5	80			
	208			

The nitro-isatoic acid melts, with decomposition, between 220° and 230°, is almost insoluble in water, difficultly soluble in alcohol, and insoluble in ether, and crystallises from a mixture of equal parts of alcohol and acetone in colourless mother-of-pearl glancing plates.

Addition of water to the solution of the acid in alcohol, or in the mixture of alcohol and acetone, precipitates it.

The chemical behaviour* of nitro-isatoic acid corresponds generally with that of isatoic acid, the former being however more stable towards water, acids, &c., than the latter. Just as isatoic acid yields with those reagents anthranilic acid, so does the nitro-derivative give a nitro-anthranilic acid, which corresponds with one of the two known compounds of this composition, and therefore explains the function of the nitroxyl in the original nitro-isatoic acid.

If the latter be evaporated along with aqueous hydrochloric acid, carbonic acid is evolved; the residue (which contains no hydrochloric acid in chemical combination) yields, upon recrystallisation from alcohol, a strong acid which forms yellow needles, is difficultly soluble in water, easily in alcohol and ether, and not at all in chloroform and benzol; it melts between 265° and 270° with decomposition.

The same acid is formed, although more slowly, by the continued boiling of a mixture of water and nitro-isatoic acid. As is shown by the properties of the product—as given above—but especially by the composition of the baryta salt prepared from it (see the analysis of the same below), we have here to do with the nitro-anthranilic acid already obtained in a different way by Hübner† and by Griess,‡ the so-called *m*-nitro-*o*-amidobenzoic acid or (ϵ-nitro-amidobenzoic acid).

The barium salt prepared from the above by means of baryta water, precipitation of the excess of barium by carbonic acid, and evaporation of the filtrate, crystallises in sharp reddish-yellow needles. According to the analysis, it is nitro-amidobenzoate of barium + 3 mol. water of crystallisation.

I. 0·2025 gram of the air-dried substance lost at 110° 0·020 gram $H_2O = 9·87$ per cent. H_2O.

II. 0·165 of the same substance lost at 110° 0·015 gram $H_2O = 9·7$ per cent.

III. 0·1825 gram of the dehydrated substance gave 0·0854 gram $BaSO_4 = 0·0502$ gram $Ba = 27·50$ per cent. Ba.

$$\text{Calculated for } \left(C_6\left.\begin{matrix}H_3\\NO_2\\NH_2\end{matrix}\right\}CO\right)_2O_2Ba + 3H_2O\ :\ H_2O = 9·76 \text{ per cent.}$$

The anhydrous salt contains $Ba = 27·45$ per cent.

If nitro-isatoic acid be suspended in a small quantity of warm water, and ammonia added, it is quickly dissolved. The solution soli-

* The chemical behaviour of nitro-isatoic acid towards water, hydrochloric acid, and ammonia, and also towards reducing agents, has been investigated by Dr. Bellmann since the death of the author.

† "Liebig's Ann. Chem.," Bd. 195, 21.

‡ "Ber. Berl. Chem. Ges.," Bd. 11, 1730.

difies soon after to a yellow crystalline mass, which is easily obtained pure by pressing and recrystallising from absolute alcohol to which some acetone has been added. The new compound, the amide of the above nitro-anthranilic acid, crystallises in yellow needles, is difficultly soluble even in hot water and in boiling alcohol, but easily in acetone.

It decomposes at a temperature between 200° and 210°. 0·236 gram of the compound dried over H_2SO_4 gave at 9° and 760 mm. bar., 45 c.c. N. N=22·93 per cent.

$$\text{Calculated for } C_6\begin{matrix}H_3\\NO_2\\NH_2\end{matrix}\Bigg\}CONH : N=23{\cdot}20 \text{ per cent.}$$

Reduction of Nitro-Isatoic Acid.

On treating nitro-isatoic acid with tin and tolerably concentrated hydrochloric acid, with gentle warming, much carbonic acid besides hydrogen escaped, which led one to suppose that a reduction product of the above nitro-amidobenzoic acid was formed, and this was verified by the examination of the resulting solution. In this there was contained the hydrochloric acid salt of a diamidobenzoic acid, which, from its relation to the above nitroanthranilic acid, may be termed amido-anthranilic acid. Its formation is shown by the following equation:—

$$C_6\begin{matrix}H_3\\NO_2\\N\end{matrix}\Bigg\}CO,COOH+H_6=C_6\begin{matrix}H_3\\NH_2\\NH_2\end{matrix}\Bigg\}COOH+CO_2+H_2O.$$

The above solution was first freed from tin by means of sulphuretted hydrogen, and then, in order to prevent decomposition setting in during the subsequent evaporation, a few crystals of acid sulphite of soda were added.* Notwithstanding this, the solution became brown-coloured, and deposited, when sufficiently concentrated, a salt likewise dark in colour, which after pressing was recrystallised from hot water containing a little hydrochloric acid. The pale-yellow salt thus got, crystallising in small prisms, has the composition of hydrochloric-diamidobenzoic acid : $C_6H_3(NH_2)_2COOH.2HCl$.

0·235 gram of the salt, dried over sulphuric acid, gave on precipitation with nitrate of silver 0·302 gram AgCl=31·79 per cent. Cl; calculated=31·51 per cent.

The sulphuric-diamidobenzoic acid,

$$C_6H_3(NH_2)_2COOH.SO_2(OH)_2,$$

prepared from the above salt, crystallises from water in hard trans-

* Compare Griess regarding his investigation upon diamidobenzoic acid. "Liebig's Ann. Chem. Pharm.," Bd. 154, 325.

parent prisms, and has, after drying over sulphuric acid, the composition given above.

0·163 gram substance gave on precipitation with chloride of barium 0·153 gram $BaSO_4$, corresponding to 32·2 per cent. SO_3; calculated 32·00 per cent.

0·324 gram of the salt gave 31·6 c.c. N at 13° and 735 mm. pressure=11·13 per cent. N; calculated 11·20 per cent. N.

Attempts to isolate the free diamidobenzoic acid from the above salts were unsuccessful; the solutions of the latter became, on addition of ammonia, of a deep indigo-blue colour. We have here in all probability before us, in the above compounds, salts of the α-diamidobenzoic acid described by Griess,* whose sulphuric acid compound crystallises free of water, and which is also obtained direct from the above described ϵ-nitro-amidobenzoic acid by reduction.

Action of Nitrous Acid on Isatoic Acid.

Into water containing finely-powdered isatoic acid in suspension, and kept at about 70°, nitrous acid was led; carbonic acid and nitrogen were evolved with foaming. From the hot filtered solution long felt-like needles separated out on cooling, and these, at first yellowish, were finally obtained colourless by several recrystallisations from hot water to which animal charcoal had been added. This compound, a strong acid, is almost insoluble in cold water, tolerably soluble in hot, and very readily in alcohol, ether, and chloroform; it melts at 228°, and can be sublimed if cautiously heated. Its water solution is coloured blood-red by addition of chloride of iron. According to analysis, it has the composition $C_7H_5NO_5$.

0·1804 gram substance gave 0·3037 gram CO_2 (0·0828 gram C), and 0·0491 gram H_2O (0·0055 gram H).

0·1894 gram substance gave at 13° and 753 mm. pressure, 12·3 c.c. N=0·0144 gram N.

	Calculated.			Found.
C_7......	84	45·90 p. c.	......	45·91 p. c.
H_5	5	2·73 ,,	......	3·02 ,,
N......	14	7·65 ,,	......	7·60 ,,
O_5......	80	43·72 ,,	......	
	183	100·00		

The melting point and other properties of the compound leave no doubt that it is the carefully investigated α-nitrosalicylic acid of Hübner—$C_6H_3(NO_2)(OH)COOH$. Its ethyl ether, prepared by means of hydrochloric acid and alcohol, crystallises in long colourless needles which melt at 95° and, according to the analysis, have the

* Journ. für Prakt. Chem." [2] 5, 237.

composition $C_6H_3(NO_2)(OH)COOC_2H_5$ (51·3 per cent. C and 4·37 per cent. H found; 51·2 per cent. C and 4·27 per cent. H, calculated). The formation of α-nitrosalicylic acid from isato acid, as described above, is easily explained upon the supposition that the anthranilic acid (amidobenzoic acid) generated from the latter is nitrated by the nitrous acid, and that in this nascent nitro-amidobenzoic acid the amide is replaced by hydroxyl.

$$\underset{\text{Nitro-amidobenzoic acid.}}{C_6H_3(NO_2)(NH_2)COOH} + NOOH = N_2 + H_2O + \underset{\text{Nitro-salicylic acid.}}{C_6H_3(NO_2)(OH)COOH}$$

The foregoing research explains the behaviour of isatoic acid substantially only in one direction. Other experiments are now being made, by means of which the changes induced in this compound by suitable oxidising and reducing agents are to be made clear, in order thereby to gain new foundations for the right perception of the chemical constitution of isatin and compounds derived from it.

We possess in the substance obtained by the oxidation of indigo, and named by me isatoic acid, a nitrogen compound which seems to be specially fitted to shed light upon the question whether nitrogen can act not only as a di-, tri-, tetra-, and penta-valent element (as in nitric oxide, ammonia, nitrous acid, and nitric acid), but also as a monovalent one. The mode of formation and the chemical behaviour of this isatoic acid gain—through the conception that it is benzoylcarboxylic acid, in which one of the five phenyl hydrogen atoms is replaced by an atom of monovalent nitrogen, and through the further supposition that isatin is its corresponding aldehyde—such a simple and, from all points of view, satisfactory explanation, that only one already prejudiced by the idea that nitrogen can only be trivalent can feel himself satisfied by the supposition that the molecular weight of isatoic acid must be doubled, i.e., *that it is a dibasic acid. None of the facts which up till now I have discovered support this hypothesis; on the contrary, by the supposition that isato acid contains* 1 *atom of monovalent nitrogen as substitute for* 1 *atom of hydrogen, they all obtain the simplest and least strained explanation in such a degree that, in the publication of my research on isatin and isatoic acid and their derivatives, I have not discussed the question whether the nitrogen in them may possibly be polyvalent.*

N.B.—Benzoyl carboxylic acid is usually termed phenyl glyoxylic acid, so that nitrogen-benzoyl carboxylic acid = nitrogen-phenyl glyoxylic acid.

III. "Abstract of some Results with respect to Doubly Periodic Elliptic Functions of the Second and Third Kinds." By JOHN GRIFFITHS, M.A. Communicated by Prof. STOKES, Sec. R.S. Received March 5, 1885.

Elliptic Functions of the Second Kind.

1. If $u=\int_0^\theta \frac{d\theta}{\sqrt{1-k^2\sin^2\theta}}$, $\mathrm{K}=\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1-k^2\sin^2\theta}}$, $\mathrm{K}'=\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1-k'^2\sin^2\theta}}$,

$k^2+k'^2=1$, $\theta=\mathrm{am}u$, $\sin\theta=\sin\mathrm{am}u$, $\cos\theta=\cos\mathrm{am}u$, $\sqrt{1-k^2\sin^2\theta}=\Delta\mathrm{am}u$, it is known that—

$$\sin\mathrm{am}(u+4\mathrm{K}+4i\mathrm{K}')=\sin\mathrm{am}u,$$

$$\cos\mathrm{am}(u+4\mathrm{K}+4i\mathrm{K}')=\cos\mathrm{am}u,$$

$$\Delta\mathrm{am}(u+4\mathrm{K}+4i\mathrm{K}')=\Delta\mathrm{am}u.$$

Similar results are true when u is replaced by the second elliptic integral $a=\int_0^\theta \sqrt{1-k^2\sin^2\theta}d\theta$, *and a corresponding change is made in Jacobi's notation.*

In fact, putting $\theta=\mathbf{am}a$, $\sin\theta=\sin\mathbf{am}a$, $\cos\theta=\cos\mathbf{am}a$, $\sqrt{1-k^2\sin^2\theta}=\Delta\mathbf{am}a$, we have the following theorems, viz.:—

$$\sin\mathbf{am}\{a+4\mathrm{E}+4i(\mathrm{K}'-\mathrm{E}')\}=\sin\mathbf{am}a,$$

$$\cos\mathbf{am}\{a+4\mathrm{E}+4i(\mathrm{K}'-\mathrm{E}')\}=\cos\mathbf{am}a,$$

$$\Delta\mathbf{am}\{a+4\mathrm{E}+4i(\mathrm{K}'-\mathrm{E}')\}=\Delta\mathbf{am}a,$$

if $\mathrm{E}=\int_0^{\frac{\pi}{2}}\sqrt{1-k^2\sin^2\theta}d\theta$, $\mathrm{E}'=\int_0^{\frac{\pi}{2}}\sqrt{1-k'^2\sin^2\theta}d\theta$, $k^2+k^{2\prime}=1$, and K, K′ are the same quantities as above.

The fundamental equations satisfied by the new functions $\sin\mathbf{am}a$, $\cos\mathbf{am}a$, $\Delta\mathbf{am}a$ are—

(1.) $\sin\mathbf{am}\{a+b-k^2\sin\mathbf{am}a\sin\mathbf{am}bf(a,b)\}=f(a,b)$,

(2.) $\cos\mathbf{am}\{a+b-k^2\sin\mathbf{am}a\sin\mathbf{am}bf(a,b)\}=\phi(a,b)$,

(3.) $\Delta\mathbf{am}\{a+b-k^2\sin\mathbf{am}a\sin\mathbf{am}bf(a,b)\}=\psi(a,b)$,

where $f(a, b)=\sin \mathrm{am}a \cos \mathrm{am}b \Delta \mathrm{am}b+\sin \mathrm{am}b \cos \mathrm{am}a \Delta \mathrm{am}a$, ÷

$$\phi(a, b)=\cos \mathrm{am}a \cos \mathrm{am}b-\sin \mathrm{am}a \Delta \mathrm{am}a \sin \mathrm{am}b \Delta \mathrm{am}b, \div$$

$$\psi(a, b)=\Delta \mathrm{am}a \Delta \mathrm{am}b-k^2 \sin \mathrm{am}a \cos \mathrm{am}a \sin \mathrm{am}b \cos \mathrm{am}b, \div$$

and common denominator $=1-k^2 \sin^2 \mathrm{am}a \sin^2 \mathrm{am}b$.

The theory of the two periods can be derived from the above and the relation

$$\int_0^{\sin^{-1}\frac{1}{k}} \sqrt{1-k^2 \sin^2 \phi}\, d\phi=$$

$$\int_0^{\frac{\pi}{2}} \sqrt{1-k^2 \sin^2 \phi}\, d\phi+i\left\{\int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1-k'^2 \sin^2 \theta}}-k^2 \int_0^{\frac{\pi}{2}} \frac{d\theta}{(1-k'^2 \sin^2 \theta)^{\frac{3}{2}}}\right\}$$

or $\displaystyle\int_0^{\sin^{-1}\frac{1}{k}} \sqrt{1-k^2 \sin^2 \phi}\, d\phi=\mathrm{E}+i(\mathrm{K}'-\mathrm{E}')$,

where $i=\sqrt{-1}$ and $\sin \phi=\dfrac{1}{\sqrt{1-k'^2 \sin^2 \theta}}$.

Elliptic Functions of the Third Kind.

2. Again, if u be replaced by $p=\displaystyle\int_0^{\theta} \frac{d\theta}{(1+n \sin^2 \theta) \sqrt{1-k^2 \sin^2 \theta}}$ and θ be taken $=\mathrm{am}p$, $\sin \theta=\sin \mathrm{am}p$, $\cos \theta=\cos \mathrm{am}p$, $\sqrt{1-k^2 \sin^2 \theta}=\Delta \mathrm{am}p$, we have

$$\sin \mathrm{am}\{p+4\Pi+4i\mathrm{P}'\}=\sin \mathrm{am}p;$$

$$\cos \mathrm{am}\{p+4\Pi+4i\mathrm{P}'\}=\cos \mathrm{am}p;$$

$$\Delta \mathrm{am}\{p+4\Pi+4i\mathrm{P}'\}=\Delta \mathrm{am}p;$$

where
$$\Pi=\int_0^{\frac{\pi}{2}} \frac{d\theta}{(1+n \sin^2 \theta) \sqrt{1-k^2 \sin^2 \theta}},$$

$$\Pi'=\int_0^{\frac{\pi}{2}} \frac{d\theta}{(1+n' \sin^2 \theta) \sqrt{1-k'^2 \sin^2 \theta}},$$

$$k^2+k'^2=1, \quad n'(1+n)=-k'^2, \quad \mathrm{P}'=\mathrm{K}'-\frac{n}{1+n}\Pi'.$$

In this case the periods depend upon the relation

$$\int_0^{\sin^{-1}\frac{1}{k}} \frac{d\phi}{(1+n \sin^2 \phi) \sqrt{1-k^2 \sin^2 \phi}}=\int_0^{\frac{\pi}{2}} \frac{d\phi}{(1+n \sin^2 \phi) \sqrt{1-k^2 \sin^2 \phi}}$$

$$+i\left\{\int_0^{\pi}\frac{d\theta}{\sqrt{1-k'^2\sin^2\theta}}-\frac{n}{1+n}\int_0^{\frac{\pi}{2}}\frac{d\theta}{\left(1-\frac{k'^2}{1+n}\sin^2\theta\right)\sqrt{1-k'^2\sin^2\theta}}\right\}$$

where $\sin\phi=\frac{1}{\sqrt{1-k'^2\sin^2\theta}}$.

3. Remarks on a $\zeta(u)$ function, which includes Jacobi's $Z(u)$ as a particular case.

If we regard p as a function of u and write $\frac{P'}{\Pi}\div\frac{K'}{K}=\mu$, there seems to be a $\zeta(u)$ relation of the form $\zeta(u)=\frac{\pi}{2(1-\mu)K'}\left\{\frac{p}{\Pi}-\frac{u}{K}\right\}$.

This satisfies the two equations

$$\zeta(u+2K)=\zeta(u),$$

$\zeta(u+2iK')-\zeta(u)=-\frac{\pi i}{K}$, as may be seen by changing (1) u, p into $u+2K$, $p+2\Pi$; (2) u, p into $u+2iK'$, $p+2iP'$, respectively.

It is easy to show by ordinary trigonometrical formulæ that we get a solution of these functional equations by putting

$$\zeta(u)=\frac{2\pi}{u_0K}\left\{\frac{q}{1-q^2}\sin u_0\sin\frac{\pi u}{K}+\tfrac{1}{2}\frac{q^2}{1-q^4}\sin 2u_0\sin\frac{2\pi u}{K}\right.$$
$$\left.+\tfrac{1}{3}\frac{q^3}{1-q^6}\sin 3u_0\sin\frac{3\pi u}{K}+\;.\;.\;.\;.\;\textit{ad infin.}\right\},$$

where u_0 is a constant and $q=e^{-\pi\frac{K}{K}}$.

The deduction of—

$$a-\frac{E}{K}u=\frac{2\pi}{K}\left\{\frac{q}{1-q^2}\sin\frac{\pi u}{K}+\frac{q^2}{1-q^4}\sin\frac{2\pi u}{K}.\;\;.\;\;.\;\;.\right\}$$

presents no particular difficulty.

This is Jacob's $Z(u)$.

The above considerations suggest the very interesting question whether we may not take u to be a function of p, and so write

$$p-\frac{\Pi}{K}u=A_1\sin\frac{\pi p}{\Pi}+A_2\sin\frac{2\pi p}{\Pi}+A_3\sin\frac{3\pi p}{\Pi}+.\;\;.\;\;.\;\;\textit{ad infin.}$$

Primâ facie this does not seem to be an impossible equation, since the series can be reversed by Lagrange's theorem, *i.e.*, we may have

$$p-\frac{\Pi}{K}u=B_1\sin\frac{\pi u}{K}+B_2\sin\frac{2\pi u}{K}+.\;\;.\;\;.\;\;.$$

If the assumption in question is valid, there follows the relation

$$1-\frac{\Pi}{K}(1+n\sin^2 \mathrm{am}p)=\frac{\pi}{\Pi}\left\{A_1\cos\frac{\pi p}{\Pi}+2A_2\cos\frac{2\pi p}{\Pi}+\ .\ \ .\ \ .\right\},$$

or, in other words, the elliptic functions of the third kind, sin am$p$, &c., can be expressed by harmonic series involving multiples of $\frac{p}{\Pi}$.

March 19, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On 'Transfer-resistance' in Electrolytic and Voltaic Cells." By G. GORE, LL.D., F.R.S. Received March 2, 1885.

(Abstract.)

The existence of this phenomenon has been a matter of doubt ever since the year 1831, and the question has been examined by many investigators. In the present paper are described a series of methods by means of which its reality has been determined. Other methods are given for measuring the amounts of such "resistance," either collectively at the two electrodes of an electrolytic cell, or separately at each electrode. Modes of obviating the interference of polarisation, and of securing success in the measurements, are also described.

The influence of various circumstances upon the phenomenon were investigated, viz., strength and density of current; total resistance; density of current and size of electrode; composition of the electrolyte; strength of ditto; combined electrolytic cells; temperature; and chemical corrosion. The relations of the phenomenon to size of plate in voltaic cells, to the positive and negative plates respectively, and to strength of current in those cells, were also examined, and the results are given.

The following are the chief facts established by this research:— That a species of electric "resistance," distinct from that of polarisation and of ordinary conduction-resistance, varying greatly in amount in different cases, exists at the surfaces of mutual contact of metals and liquids in electrolytic and voltaic cells. That this "resistance"

varies largely in amount with different metals in the same solution, and with the same metals in different solutions; in dilute solutions of mineral acids of different strengths, or of different temperatures, and is usually small with easily corrodible metals which form quickly soluble salts, and large with those which are not corroded; and is disguised in the case of those which by corrosion form insoluble salts.

The results of the experiments also show that the same voltaic current was "resisted" in different degrees by every different metal when employed as an anode, and when used as a cathode; also by the same metal when used as an anode and cathode respectively; and that the proportions of such "resistance" at an anode and cathode of the same metal, varied with every different metal in every different electrolyte (and strength of electrolyte), and at every different temperature; and that the resistance at the anode was usually smaller than that at the cathode; in some cases, however, where a film was formed upon the anode, an apparently reverse effect occurred; that a current from a given positive plate of a voltaic cell was differently resisted by every different metal used as a negative plate in that cell; and that by rise of temperature "transfer-resistance" was usually and considerably reduced.

They further show that this species of "resistance" was largely reduced by increasing the strength of current; and was thus conspicuously distinguished from ordinary conduction-resistance of the electrolyte. In consequence of this effect, "transfer-resistance" was greatly influenced by every circumstance which altered the ordinary resistance, and thereby the strength of current. The usual effect of diminishing the density of current alone, by enlarging both the electrodes and keeping the strength constant, was to diminish the "transfer-resistance;" and of enlarging one only, was to diminish it at that electrode and increase it at the other, the effect being greatest at the altered electrode; but the influence of density was very much smaller than that of strength of current. The current was usually less "resisted," and larger with a small positive plate and a large negative one, than with those sizes reversed. Alterations of size or kind of metal at one plate of an electrolytic or voltaic cell affected the "transfer-resistance" at the other, by altering the strength and density of the current.

"Transfer-resistance," therefore, appears to vary, not only with every physical and chemical change in the metals and liquids, but also with every alteration in the current. Such "resistance" throws light upon the relative functions of the positive and negative plates of voltaic cells, and illustrates the comparatively small influence of the negative one in producing strength of current. Nearly all these conclusions are based upon results represented by average numbers obtained by series of experiments.

II. "Note on Rev. Robert Harley's paper, 'Professor Malet's Classes of Invariants identified with Sir James Cockle's Criticoids." By JOHN C. MALET. Received March 7, 1885.

In 1882, a paper of mine "On a Class of Invariants" appeared in the "Philosophical Transactions," in which I used, for the determination of theorems, two classes of functions of the coefficients of linear differential equations. In consequence of a communication from the Rev. Robert Harley, I appended to the paper the following note:—

"Since the publication of the abstract of this paper, the Rev. R. Harley has mentioned to me that the first class of functions treated of here have [has] been already investigated by Sir James Cockle; having consulted the memoirs I was referred to by Mr. Harley, I think little similarity will be found between Sir James Cockle's results and mine.—J. C. M."

One omission I certainly made,* through ignorance, in this note, and I regret it; I did not notice that the second class of functions had also been treated of by Sir James Cockle; this omission, however, appears to me to be a slight one, for anyone treating of the first class would almost as a matter of course be led to treat of the second also, and the existence of the functions is so obvious as hardly to need proof, and the determination of them was with me a process of calculation carried on as far as was necessary for the purposes of my paper.

As far as concerns the mere existence of these functions, certainly those of the first class, the credit of discovering them might be fairly claimed for the writers who first pointed out the strict analogy that exists between linear differential equations and ordinary algebraic equations, for when the second term of an equation is removed, the new coefficients will of course be functions of the old. It never, therefore, occurred to me that anyone reading my note would suppose that I there meant to assert that the functions previously treated of by Sir James Cockle were not identical with those I made use of, and that the latter part of my note referred to the mere calculation of them.

However, more than two years after the publication of my paper, Mr. Harley communicated to the Royal Society the paper mentioned in the heading of this note, and which I have just seen in the number of the "Proceedings" recently published. In this paper Mr. Harley says:

"Professor Malet says that having consulted the memoirs to which I referred him, he thinks 'little similarity will be found between Sir

* Due, no doubt, to oversight on my part.—J. C. M.

James Cockle's results' and his own. The object of this communication is to show that there is not only similarity but absolute identity, the two classes of functions considered by Professor Malet coinciding in every point with the ordinary and differential criticoids discussed by Sir James Cockle."

My object in writing this note is to call attention to the fact that, by the omission of the first part of my note, and his own comments on the partial extract he makes from it, Mr. Harley represents me as making a statement bearing an interpretation very different from that I meant it to bear.

Having done so, I will trouble the Society with the matter no further, and will leave it to those who may be interested, to judge if the general results of my paper are identical with Sir James Cockle's.

III. "The 'Paralytic' Secretion of Saliva." By J. N. LANGLEY, M.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. Received March 16, 1885.

It was shown by Claude Bernard that section of the chorda tympani nerve in the dog, causes, after an interval of about twenty-four hours, a slow "paralytic" secretion of saliva from the sub-maxillary gland; the secretion continues for several weeks, and is accompanied by a gradual diminution in the size of the gland. Heidenhain confirmed these observations, and he found further that the effect was not confined to the gland on the side of the body on which the nerve had been cut, but extended also to the corresponding gland of the opposite side of the body, so that section of *either* chorda tympani nerve caused a continuous secretion from *both* sub-maxillary glands. Since Heidenhain's paper in 1868, nothing has, so far as I know, been published on this subject. I purpose to give a brief account of some observations which were made by me several years ago, and which may serve to recall attention to certain curious facts touching both nerve and gland physiology.

Since the secretion, which takes place on the side of the body on which the nerve is cut, is called the "paralytic" secretion, we will call the corresponding secretion, which takes place on the opposite side of the body, the "anti-paralytic," or more briefly the "antilytic" secretion.*

I will consider first the paralytic and antilytic secretions during the first day or two of their occurrence. During this time the

* A fuller account will be published in the forthcoming number of the "Journal of Physiology."

paralytic secretion is stopped for several hours at least, by cutting the sympathetic nerve-fibres running to the gland; the antilytic secretion is made slower by cutting the chorda tympani, and is stopped by cutting, in addition, the sympathetic fibres on the carotid, that is, the secretion ceases in each case when the nervous connexions between the gland and the central nervous system are severed. From this it follows that the paralytic secretion in its early stage is caused by nervous impulses passing from the central secretory centre down the sympathetic nerve to the gland, and that the antilytic secretion is similarly caused by nervous impulses sent out from the central secretory centre, but passing in part down the chorda tympani nerve which is here intact.

Since the paralytic secretion is more copious than the antilytic secretion, it follows that the nervous impulses sent out by the secretory centre on the side of the body on which the chorda tympani is cut, are of greater intensity than those sent out by the secretory centre of the opposite side. Thus, section of one chorda tympani is followed by a change in the central secretory centre of such a nature, that it continuously sends out nerve-impulses tending to produce a secretion from the sub-maxillary glands; the change, however, is not equal on the two sides, but is more profound on the side of the body on which the chorda tympani has been cut.

That the central nerve-cells concerned in producing the secretion are not in their normal condition, can be shown in another way. It is well known that in a normal animal, dyspnœa causes, when the chorda tympani nerve is intact, a secretion of saliva from the submaxillary gland. Now, in the stage of the paralytic and antilytic secretion spoken of above, when they are produced by stimuli sent out by the central nervous system, dyspnœa causes a much more rapid flow of saliva, and causes it sooner than it does normally; moreover, the effect of dyspnœa is greater on the paralytic than on the antilytic secretion. Hence, then, section of one chorda tympani causes an increase of irritability in the central secretory centre, the increase of irritability being greater on the side of the body on which the chorda tympani nerve has been cut.

Since a venosity of blood greater than normal, will, in a normal animal, serve as a stimulus to the central nerve-cells, and cause a flow of saliva, it is probable that if the irritability of the nerve-cells be increased, the normal venosity of the blood will serve as a stimulus to the nerve-cells, and cause a flow of saliva. Hence I think it is not unreasonable to suppose that the paralytic and the antilytic secretions in their early stages are essentially similar to the dyspnœa secretion of the normal animal, and that they are proximately caused by the central nerve-cells, in their state of increased irritability, being stimulated by the blood supplied to them. This view is confirmed by

the effect of apnœa; during apnœa both the paralytic and the antilytic secretions stop.

The antilytic secretion is, as far as I have observed, solely of central origin, since it ceases on severing the nerves connecting the gland with the central nervous system; according to Heidenhain, however, it continues after the nerves have been severed.

The paralytic secretion is only in its early stage of central origin, very soon local changes come into play, rapidly increase in intensity, and continue long after the central changes have ceased to be effective to produce a secretion. Thus, then, the paralytic secretion in its later stages continues undiminished after section of all the sympathetic fibres running to the gland. When the chorda tympani and sympathetic fibres are simultaneously cut, the paralytic secretion which follows is of course of local origin only. There are two ways in which the secretion of local origin might be brought about, either by a change in the gland-cells of such a nature that in their abnormal nutritive conditions they secrete continuously, or by a change in a local secretory centre analogous to that which takes place in the central secretory centre. Heidenhain is inclined to adopt the former view, and, on general grounds, it does not seem to me unlikely that gland-cells, normally secreting in response to nervous impulses only, should in certain circumstances secrete continuously without such impulses; but in this particular case there are I think fair grounds for believing that the secretion is caused by nervous impulses sent out by a local secretory centre.

These grounds I will briefly state. On the course of the nerves between the lobes and lobules of the gland there are many nerve-cells. It is highly probable that some at any rate of these nerve-cells are connected with the secretory nerves, since the chorda tympani nerve-fibres, unlike the nerve-fibres of the skeletal muscles and those sweat glands, degenerate very slowly after severance from the central nervous system. In the cat the chorda tympani fibres on the duct, near its entrance into the sub-maxillary gland, produce a secretion when stimulated, two to three weeks after the nerve trunk has been cut.*

Further, the secretion of local origin is increased by dyspnœa, is stopped by apnœa, and by anæsthetics when given in considerable excess. That is, the paralytic secretion, when it is produced by

* Pilocarpin causes a secretion for more than six weeks after section of the chorda tympani, but no conclusion with regard to the condition of the nerves can at present be drawn from this, since it is possible that pilocarpin acts directly on the gland-cells.

During the paralytic secretion produced by section of the chorda tympani, stimulation of sympathetic nerve causes a flow of saliva very much as if the gland were normal.

changes occurring locally, is affected by dyspnœa, by apnœa, and by anæsthetics in the same way as it is when produced by nervous impulses sent out from the central secretory centre; and this strongly suggests that the secretion in the former case also is brought about by nervous impulses, proceeding in this instance from a local secretory centre.

I conclude, then, that section of the chorda tympani causes an increase in irritability, both in a central and in a local secretory centre, during which increase of irritability the blood passing through the centre serves as an effective stimulus. The central centre in no very long time recovers its normal state, the local centre does not. Probably it and the gland eventually atrophy unless the chorda tympani regrows.

Although the secreting cells of the sub-maxillary gland steadily diminish in size during the paralytic secretion, they undergo only slight histological changes; they become somewhat more mucous. The demilune cells and the serous cells, which are present in considerable number in the sub-maxillary gland of the cat, do not show any obvious change, except their diminution in size; the increase in the number of these cells, which is stated by Heidenhain to occur, I have not observed. That the cells, in spite of the paralytic secretion, are in a "resting" and not in an "active" condition, is further shown by the cells being, in the fresh state, granular throughout. On the side of the antilytic secretion the gland-cells are rather less mucous than in the normal "resting" gland.

March 26, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Chairman announced that Mr. Walter White, after more than forty years of faithful service, had retired from the office of Assistant Secretary, and that Mr. Herbert Rix had been appointed to fill the vacancy.

The following Papers were read:—

I. "Observations on Variations of the Electromotive Force between Metals at High Temperatures in Fused Salts." By THOMAS ANDREWS, F.R.S.E., F.C.S. Communicated by Sir HENRY ROSCOE, F.R.S. Received March 12, 1885.

(Abstract.)

Reversals of the electromotive force between platinum and other metals in fused salts appear to have been noticed by Andrews about the year 1837, and in 1858 Hankel made some observations in this direction (Hankel, "Poggendorff's Annalen," 103, p. 612, 1858). Dr. Gladstone and Mr. Tribe ("Phil. Mag.," 1881) also found that a strip of silver plunged in molten AgI or AgCl, gives rise to a growth of silver crystals; the cause of this separation is the dissimilar temperature of different portions of the strip which produces thermo-currents. The present communication is an attempt to obtain quantitative estimations of the E.M.F., and of the extensive deviations from the normal electro-chemical positions of the metals (platinum and copper and platinum and iron) in fused salts, and the conditions of high temperature attending these reactions, which do not appear to have been previously determined. The cell for fusing consisted of a large platinum crucible (surrounded by known high temperatures) forming one element, a thick bent copper or iron rod inserted in the fused salt forming the other, or more frequently the copper plug of the Siemens' water pyrometer (which had been used in taking the time changes of temperature) was thus employed. A delicate galvanometer of known constants was used for taking the E.M.F., which was calculated from the observed deflections, in conjunction with the

ascertained resistances of the fused salts. The temperatures were determined in a large number of observations by the aid of a Siemens' water pyrometer. The salts employed as electrolytes were potassium carbonate, potassium chloride, potassium nitrate, potassium chlorate, potassium bisulphate, and sodium chloride, using platinum and copper or iron as elements. The action of two dissimilar salts in contact during fusion at 845° C. (K_2CO_3 and NaCl) on the same metal, platinum, was also observed, and considerable reversals of the E.M.F. to the extent of 0·37 volt occurred apparently from divergence of temperature in the relative rate of cooling from fusion of the two salts.

Table A* contains quantitative estimates in volts of the deviations of the E.M.F. from the normal. The results on this Table A show that by a regulation of the heat between the metals forming the elements, extensive deviations from the normal electro-chemical positions of the metals were obtainable, in connexion with some of the above fused salts, under the conditions of temperature recorded on the Tables A and B. In K_2CO_3, fused and resolidified (being then under the fusing point), a reverse E.M.F. of 1·037 volts was noticed (platinum positive), the current flowing from the platinum to the copper; on remelting the salt and equalising the temperature throughout to 845° C., an instant reversal of the direction of the current took place, the metals resuming their normal positions with an E.M.F. of 0·22 volt, platinum being now negative. In the case of platinum and iron, under the same conditions in the above salt, a total deviation of about 0·88 volt from the normal position was obtainable (platinum positive). In the water tube experiments greater divergences were noticed (see Table B). With potassium chloride, interchanges of position between the metals occurred, platinum at first being positive, with an E.M.F. of 0·318 volt, representing a total deviation of 0·94 volt. With potassium nitrate just before fusion point, an E.M.F. of 0·088 volt was observed (platinum positive); on the salt, however, reaching fusion, a reversal took place, the copper assuming its normal positive position.

Table B contains estimates of the temperature conditions attending the variations of the E.M.F. in fused K_2CO_3. Considerable reversals of the E.M.F. were noticed (platinum positive). The experiments in columns 1 and 3 show that a temperature divergence of about 260° C. between the platinum and copper, gave a reverse E.M.F. of 0·44 volt (platinum positive), a difference of 265° C. between platinum and iron gave a reverse E.M.F. of 0·24 volt (platinum positive), the E.M.F. reducing to a certain extent as the temperature difference decreased. To obtain a greater difference of temperature between

* The Tables A and B are not given in this abstract.

the metals (see columns 2 and 4), the copper element consisted of a bent pipe, immersed in the fused salt, through which water was kept flowing, the temperature of the copper being about 29° C. The platinum crucible was surrounded by temperatures from 549° to 879° C., a total variation of 850° C. between the two elements was obtained. A similar method was adopted in the platinum and iron experiments. A temperature divergence of about 518° C. between platinum and copper gave a reverse E.M.F. of 0·92 volt (platinum positive), but with the salt in liquid fusion (platinum at 879° C., copper at 29° C.) a reverse E.M.F. (platinum positive) of only 0·66 volt was obtainable. A temperature variance of about 522° C. between platinum and iron yielded an E.M.F. of 0·53 volt (platinum positive), but when the salt was in liquid fusion (platinum 879° C., iron 29° C.) a reverse E.M.F. (platinum positive) of only 0·40 volt was noticed. In a cell of this description, two opposing forces were observed in operation, the thermo-electric, contending at the higher temperatures with the normal electro-chemical action of the fused salt. The results of the water pipe experiments, recorded in detail on Table B, show that generally from some cause the extent of the reversed E.M.F. did not appear to correspond in exact proportion with the temperature divergences between the metals; this result may perhaps be accounted for by the electrolyte at the increased temperatures combating the thermo-electric influences, and commencing to restore the true electro-chemical equilibrium of the metals. A point of temperature is reached where the thermal effects counterbalance the normal electric action of the fused salt. There is apparently a powerful thermic influence, where equality of temperature does not obtain, reversing the E.M.F. from the metals, notwithstanding their immersion at high temperatures in such electrolytes as fused salts; this reversal of direction of the current in the case of K_2CO_3, continuing even with the salt at a temperature of 695° C., or above, the current passing from the hotter platinum to the colder copper. When, however, the point of fusion of the hot solidified salt is reached (834° C.), the metals, being at an uniform temperature therein, resume their true electro-chemical positions.

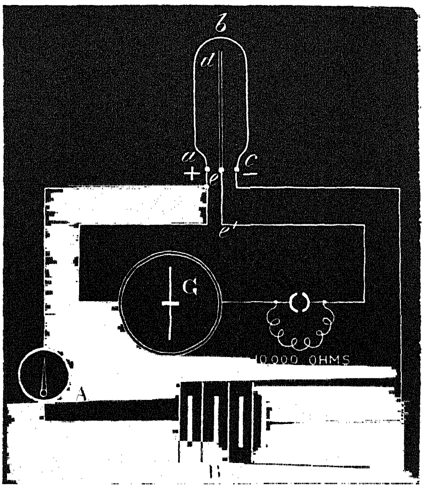
The foregoing and other repeated experiments appear to indicate that in the form of apparatus used by the author, these interchanges in the direction of the current between platinum and copper, or platinum and iron, were almost solely caused by differences of temperature surrounding the two metals forming the elements.

II. "On a Peculiar Behaviour of Glow-Lamps when raised to High Incandescence." By WILLIAM HENRY PREECE, F.R.S.

Received March 18, 1885.

1. During my recent visit to America (October, 1884) Mr. Edison showed me a very striking experiment with glow-lamps, the principle of which he had not threshed out, although he had attempted to apply it practically to the regulation of the current flowing in electric light circuits.

FIG. 1.



If *abc* be the incandescent filament of a glow-lamp, *de* a thin narrow platinum plate fixed between the limbs of the filament with an independent wire connexion *ee'* sealed in the glass globe, then, if a galvanometer G be connected between *a*, the positive electrode, and *e*, a derived current will be observed to pass through G, and through the rarefied space *ec* when the main current is increased to a certain strength, and the filament reaches a certain degree of incandescence. The strength of this derived current will increase with the increased brilliancy of the glowing filament. Mr. Edison made for me several lamps of different forms and character to enable me to investigate the phenomenon more carefully in England, and I have the pleasure of submitting the results of those experiments to the Society.

2. I used 60 Faure-Sellòn-Volckmar cells freshly and fully charged up at each series of experiments. The current through the filament was measured by a specially constructed and calibrated Ayrton and Perry direct-reading spring ammeter. The galvanometer in the shunt circuit was a sensitive tangent galvanometer of the Post-office pattern. The current through the filament was regulated by varying the number of cells. The current and electromotive force through the shunt *aec* were calculated by the following method:—The resistance of the galvanometer G was 1070 ohms, and a variable resistance R was inserted in its circuit. When the current from a standard Daniell cell (1·07 volt) is sent through the galvanometer, we get a tangent reading d, which, since $\frac{1·07}{1070} = ·001$, is the deflection, or tangent reading corresponding to 1 milliampère of current. The deflection (d_1) given by the shunt current was first read without any resistance being inserted.

Hence

$$C=\frac{d_1}{d} \text{ milliampères.}$$

Resistance was then added and a second reading d_2 taken; then the resistance r of the shunt is obtained from the formula

The electromotive force producing the current is

$$E=Cr.$$

The shunt circuit includes the rarefied space *ec*, and it is the resistance of this space that we desire to know.

All the observations were simultaneously made by different observers. While one observed the behaviour of the lamp, another read the current flowing through the filament, a third read the currents in the shunt, a fourth varied the electromotive force, and a fifth recorded the results. The electromotive force of the cells was carefully measured before and after the experiments. No photometric measurements were attempted.

3. *Experiment* 1.—The connexions were made as shown in fig. 1. The lamp (No. 4) was a short (75 mm.) filament lamp, with a platinum plate 30 mm. long and 5 mm. broad.

The variation of the current and the increase in the resistance of the filament towards the end of the experiment, together with the behaviour of the shunt, are very noticeable. It is quite clear that when the electromotive force attained 82 volts, a critical point was

Experiment 1.

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Resistance.	
		ohms.	volts.			
44·40	0·7	63·4				
46·62	0·75	62·17	..	..	..	Normal incandescence, viz., 8 candles.
48·84	0·81	60·30				
51·06	0·86	59·37				
53·28	0·91	58·55	14·7	0·30	49,000	
55·50	0·97	57·21	..	..	..	Diffused blue effect in globe appeared.
57·72	1·02	56·60	16·6	1·15	14,500	
59·94	1·08	55·50	16·2	1·80	9,000	
62·16	1·13	55·01	13·5	2·70	5,000	
64·38	1·20	53·66	11·95	3·85	3,000	
66·60	1·24	53·71	7·45	5·25	1,400	Blue effect brilliant.
68·82	1·24	55·51	10·00	5·00	2,000	
71·04	1·30	54·65	9·10	6·00	1,300	
73·26	1·35	54·28	6·50	5·00	1,300	
75·48	1·35	55·92	9·35	5·50	1,700	
77·70	1·41	55·12	9·60	6·00	1,600	
79·92	1·47	54·38	9·80	7·00	1,400	
82·14	1·60	51·34				Deflection too unsteady for observation. Blue effect very brilliant.
84·36	1·60	52·74	critical point			
86·58	1·53	56·59				
88·80	1·56	57·00	50·6	2·20	23,000	
91·02	1·53	59·30	62·5	2·50	25,000	
93·24	1·57	59·40	61·0	3·20	19,000	
95·46	1·60	59·51	95·0	3·80	25,000	
97·68	..	..	..	..	..	Filament broke. Interior of globe and faces of plate blackened.

reached. From that point the current in the filament remained very steady, but the resistance gradually increased. The shunt increased in resistance enormously, and the current through it diminished, although the electromotive force increased very largely. It is remarkable how steady the electromotive force in the shunt remained until the critical point was reached, when it suddenly increased and only reached that of the main current at the point of rupture of the filament. Again the current, which steadily increased until the critical point was reached, then diminished, indicating a considerable increase in the resistance of the rarefied space *ec*. In one subsequent experiment, after the critical point was reached, no current could be obtained through the shunt.

The direction of the current is shown in the figure.

Towards the end of the experiment, when the characteristic diffused blue effect in the globe was very marked, a bright arc was observed to be playing about the bottom of the limb at *c*, and it was quite clear that a bridge of conducting material was formed between *e* and *c*, which, together with the galvanometer, made a shunt to the filament.

This experiment was repeated upon different lamps, and the results were so similar that it is not necessary to reproduce the observations.

In all cases an intimate connexion was observed between the blue effect and the appearance of the shunt current.

4. One of Mr. Edison's assistants showed me in Philadelphia that while the effect was very perceptible when the connexions were made as shown in fig. 1, *a* being in connexion with the positive and *e* with the negative pole, no shunt current or a very slight one could be observed when the direction of the current was reversed. I did not find this invariably the case. One lamp (No. 8) only—a long filament lamp (150 mm. long)—which gave a marked shunt current and the blue effect, when the connexions were made as in fig. 1 failed to give any current or blue effect when the current was reversed within the limits of the electromotive force at my command. Doubtless I should have got both effects if I could have raised the electromotive force. In all cases, however, the effects appeared sooner and were more marked when the connexions were as shown in fig. 1 than when the direction of the current was reversed. The effect of placing the galvanometer between *e* and *c* was the same as reversing the current.

5. As the effect might be due in some way to the material of the conducting plate (*de*) inserted between the limbs of the filament, Mr. Edison made for me lamps with copper, iron, and carbon plates.

The following experiments (pp. 223 and 224) were then made, the connexions being the same as fig. 1.

No marked difference was thus observable.

If we examine the shunt current when the faint blue tinge appeared, it was: with carbon, 3·42; with iron, 5·85; and with copper, 3·80 milliampères.

6. It might be affected by the extent of surface of the metal plate, therefore lamps were made with a plate of fine wire, and also of a very broad surface, but no difference was observable between these and the normal plate used.

Experiment 2.—Carbon Centre (single).

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Resistance.	
		ohms.	volts.			
80	0·50	160·0				
84	·57	147·4	..	0·98		
88	·60	146·7	5·44	0·54	99,000	
92	·65	141·6	4·76	1·22	39,000	
96	·70	137·1	27·7	2·20	12,600	
100	·74	135·1	25·4	3·42	7,420	Blue tinge appeared.
104	·79	131·7	21·4	4·88	4,380	
108	·83	130·1	17·5	7·32	2,390	
88	0·60	146·7	08·23	0·46	179,000	Reversed the current through the filament and repeated the experiments.
92	·65	141·6	04·86	0·93	52,300	
96	·70	137·1	26·0	1·76	14,800	
100	·74	135·1	21·1	2·93	7,190	Blue tinge appeared.
104	·79	131·7	19·1	4·45	4,300	
108	·83	130·1	17·5	7·32	2,390	

Experiment 3.—Iron Centre (single).

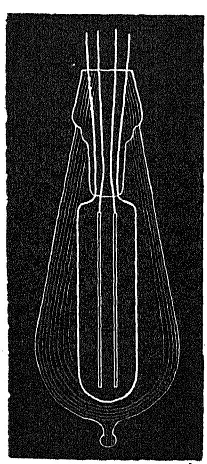
Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Resistance.	
		ohms.	volts.			
80	0·50	160·0				
90	·57	157·9				
96	·62	154·8	..	0·25		
100	·66	151·5	..	0·49		
110	·77	142·9	31·4	2·05	15,300	
120	·87	137·9	19·8	5·85	3,380	Faint blue.
90	0·57	157·9	..	0·09	..	Reversed the current through the filament and repeated the experiments.
100	·66	151·5	..	0·49		
110	·77	142·9	27·3	1·95	14,000	
120	·87	137·9	20·0	5·61	3,560	Faint blue.

Experiment 4.—Copper Centre (single).

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Resistance.	
		ohms.	volts.			
80	0·57	140·4	..	0·048		
90	67	134·3	39·2	1·12	35,000	
100	79	126·6	19·0	3·80	5,000	Faint blue.
110	90	122·2	..	needle hard over	..	Strong blue.
90	0·67	134·3	29·9	0·88	34,000	Reversed the current through the filament and repeated the experiments.
100	·79	126·6	17·2	3·32	5,190	Faint blue.
110	·90	122·2	..	needle hard over	..	Strong blue.

7. Lamp No. 9 was made with a double platinum plate, thus:—

Fig. 2



The following experiment shows that no perceptible difference was observable:—

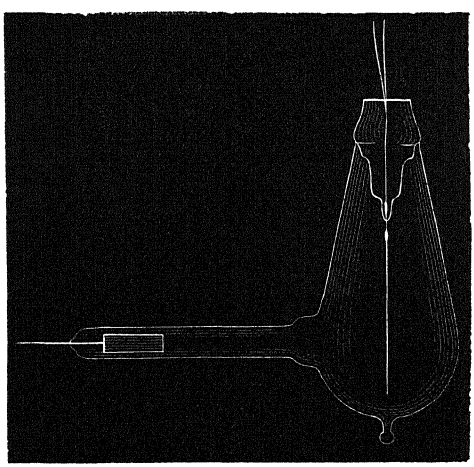
Experiment 5.—No. 9 Lamp (double platinum).

Filament.			Shunt.						Remarks.
			Calculated E.M.F.		Current in milliampères.		Ohms.		
Volts.	Ampères.	Calculated resistance (hot).	Left.	Right.	Left.	Right.	Left.	Right.	
		ohms.	volts.	volts.					
80	0·56	142·9	5·46	5·46	0·39	0·39	140,000	140,000	
84	·60	140·0	3·8	3·8	0·83	0·83	45,700	45,700	
88	·65	135·4	24·5	24·5	1·56	1·56	15,700	15,700	
90	·68	132·4	20·5	20·5	2·05	2·05	10,000	10,000	
94	·72	130·6	16·2	16·2	3·27	3·27	4,950	4,950	
100	·79	126·6	15·4	15·4	5·85	5·85	2,630	2,630	Slight blue tinge perceptible.
110	·92	119·6			Doubt	ful.			

8. Although the maximum effect was produced by Mr. Edison when the plate was fixed between the limbs, he obtained a current when it was fixed in any part of the rarefied space. If the effect was due primarily to the Crookes effect, or to the projection of molecules from the carbon filament on to the metal plate, since this bombardment takes place in right lines, we ought to have obtained effects when these lines were projected on the plate, but no effects when they could not strike the plate. Several lamps were made, which are shown in the following sketches.

9. The metal plate was taken from between the limbs of the filament, and placed at the end of a tube which had a portion of the filament exposed to the plate.

FIG. 3.



Experiment 6.—No. 5 Lamp.

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères	Ohms.	
		ohms.	volts.			
50	..	..	..	..	..	$a+$, $c-$, Fig. 1.
60						
70	0·55	127·3				
80	0·65	123·1				
84	0·70	120·0				
86	0·73	117·8				
88	0·75	117·3				
90	0·77	116·9				
92	0·80	115·0				
94	0·82	114·6				
96	0·85	113·0		No current.		
98	0·87	112·6	..	..	..	Blue effect visible in bulb.
100	0·89	112·4				
102	0·93	109·7				
104	0·96	108·3				
106	0·99	107·1	..	..	..	Blue effect strong in bulb; none in tube.
108	1·01	106·9				
110	1·05	104·7				

Recommenced Experiment with 100 volts.

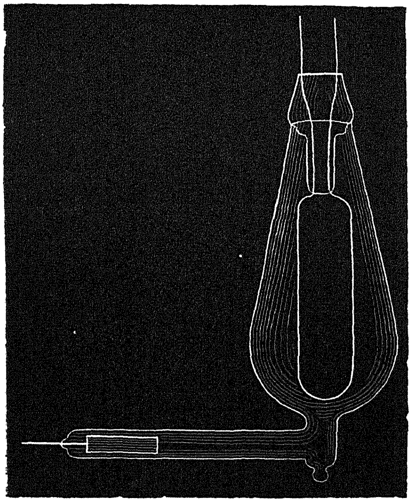
Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Ohms.	
		ohms.	volts.			
100	0·89	112·4	..	..	..	$a+$, $c-$, fig. 1.
102	0·93	109·7				
104	0·96	108·3	..	·049		
106	0·99	107·1	..	·049		
108	1·01	106·9	..	..	..	Blue effect entering tube.
110	1·05	104·7	..	·073		
112	1·07	104·7	..	·146		
114	1·11	102·7	..	·195		
116	1·14	101·8	..	·24		
118	1·18	100·0	11·3	·39	29,000?	Deflection too low to enable resistance to be properly measured.
120	1·22	98·36	10·9	·49	22,300	

Reversed the current through the filament. Returned to 90 volts, and repeated the experiments. Filament current readings the same as before, and also the blue effects. At 108 volts a slight blue effect was noticed in the open end of the tube.

Doubtless, if I could have increased the electromotive force, the results would have been more marked; but they were sufficient to show that the effects were evident, even though the rarefied space were greatly extended, as in the case of the tube attached to No. 5 lamp.

10. The tube was constructed so that no portion of the filament was opposed by right lines to the metal plate (fig. 4).

FIG. 4.



11. A lamp was constructed with three branches at right angles to each other, as shown in fig. 5, and each metal plate taken in succession, but no results were obtained.

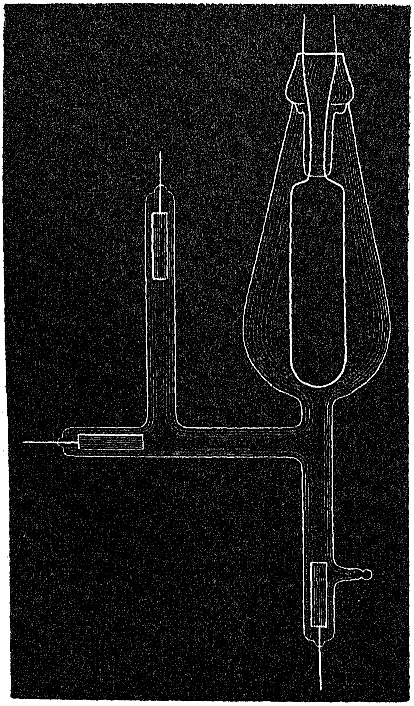
12. Professors Liveing and Dewar ("Proc. Roy. Soc.," March 9, 1882) observed a "sort of flame" during high incandescence, showing by its spectrum the presence of carbonic oxide. It was strongest about the junction of the carbon thread and the positive electrode. It was, according to them, the glow of the positive pole attending a discharge in rarefied gas.

It is a common thing with glow-lamps which have the heels of the filament close together to have an arc forming across when the electromotive force at the terminals is too high. Hence in recent lamps requiring 100 volts, Mr. Swan has considerably increased the distance between the electrodes. Moreover, whenever the incandescence of

Experiment 7.—No. 6 Lamp (long filament).

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.F.M.	Current in milliampères.	Ohms.	
		ohms.				
76						
80	0·57	140·3				
84	0·60	140·0				
88	0·65	135·4				
92	0·71	129·6				
96	0·75	128·0				
100	0·80	125·0	..	..	..	Faint blue in globe.
104	0·84	123·8	..	·049	Deflections too low to enable resistance to be measured.	
108	0·89	121·3	..	·085		
112	0·94	119·1	..	·10		Blue in globe very marked.
116	0·99	117·2	..	·073		
120	1·06	113·2	..	·20		Bulb hot, tube cool.

Fig. 5.



Experiment 8.—No. 7 Lamp.

Filament.			Shunt.			Remarks.
Volts.	Ampères.	Calculated resistance (hot).	Calculated E.M.F.	Current in milliampères.	Ohms.	
		ohms.				
80	0·56	142·9				
90	0·66	136·4				
100	0·77	129·9	..	..	..	Blue effect in globe visible.
102	0·79	129·1		No current evident in	either section.	
104	0·82	126·8				
106	0·84	126·2				
108	0·86	125·6				
110	0·89	123·6				
112	0·91	123·1				
114	0·94	121·3				
116	0·98	118·4				
118	1·00	118·0				
120	1·02	117·7				

the filament is raised beyond a certain limit the interior of the glass envelope is blackened by a layer of carbon, which has been deposited by a Crookes bombardment effect. When the carbon filament is fixed on copper electrodes, the interior of the glass sometimes becomes coated with copper as well as with carbon, and the line between the two is perfectly marked, showing that the bombardment takes place in right lines. Experiment 1 shows how very high the electromotive force can be carried, if it be steadily and rapidly increased, before the filament is broken; but practice shows that when once the blue effect appears, destruction is only a question of time. Hence the blue effect is an indication of the advent of disintegration, and a very useful warning of danger ahead.

13. Now it is clear that we have a combination of the phenomena above described in the Edison effect. A continuous bridge of molecules is formed between the junction of the carbon filament and the metal plates inserted between its heels. They are found deposited on the metal plate. A shunt is thus formed whose resistance is measurable, and a definite current passes. This shunt is formed just where the negative metallic connexion joins the heel of the carbon filament, as we should expect from the investigations of Mr. Crookes. The current is, however, weak and variable, and it is scarcely reliable enough to be useful for practical purposes as was hoped by its discoverer. When the critical point is reached the blue glow and flame seem to pervade the whole bulb, and the arc-like effect, instead

of playing about the heel, surrounds apparently the whole filament. The result is that the current passes through the galvanometer and through the rarefied space. This is clearly shown in Experiment 1.

14. It is quite clear that the critical point is reached when the filament commences to be disintegrated by the projection of its molecules from its surface. It is here that the resistance of the filament commences to increase, and the law of radiation and light emission ("Proc. Roy. Soc.," No. 229, 1884) commences to be departed from, as was shown by me in a paper read before the British Association at the Montreal meeting.*

15. It is very evident that this Edison effect is due to the formation of an arc between the carbon filament and the metal plate fixed in the vacuous bulb; that this arc is due to the projection of the carbon particles in right lines across the vacuous space; and that it makes its appearance earlier, and is more strongly marked, when the connexions are as shown in Fig. 1 than when they are reversed, because, as Mr. Crookes has pointed out, the projection proceeds from the negative to the positive pole, and it would commence at the point of least resistance. Its presence is detrimental to the life of the lamp, and as its appearance is contemporaneous with the blue effect, the latter is a warning of the approach of the critical point, and a sure indication that the electromotive force is dangerously high. It is also clear that as the Edison effect is only evident when we are "among the breakers," it is not available for practically regulating the conditions of electric light currents as its ingenious discoverer originally proposed.

* In this paper I pointed out from experimental data that the light emitted by a glow-lamp varied apparently as the sixth power of the current. I verified this law, not only by subsequent experiments of my own, but, which is much more satisfactory, by experiments of others. Professor Kittler, of Darmstadt, and Captain Abney made, independently of each other, most careful and exhaustive measurements in this direction. I tabulated and traced them out in curves. They fully confirm the law that

$$L = kC^6,$$

but within limits, and that these limits embrace the ordinary range of a glow-lamp when used for artificial illumination. As long as the resistance and the current vary uniformly together the law holds good; but as the state of incandescence is increased, a point is reached, varying with each kind of lamp, when the resistance ceases to diminish at the same rate, and eventually increases. When this occurs the law is departed from, and the light emitted increases less than the sixth power of the current. The filament speedily breaks. The point of departure from the law indicates a point when a change of state occurs in the carbon filament. Disintegration probably sets in. This point ought to be determined for each kind of lamp, and it should never be allowed to be reached, for it is from this point that decay commences and rupture follows.

The Society then adjourned over the Easter Recess to Thursday, April 16th.

Presents, February 5, 1885.

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April 16, 1885.

THE TREASURER in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Note on an Experiment by Chladni." By CHARLES TOMLINSON, F.R.S. Received March 14, 1885.

[PLATE 1.]

Lord Rayleigh, in a memoir "On the Circulation of Air in Kundt's Tubes," &c., remarks ("Proc. Roy. Soc.," xxxvi, 10, and "Phil. Trans.," 1884, Part I, p. 1) that "it was discovered by Savart that very fine powder does not collect itself at the nodal lines, as does sand in the production of Chladni's figures, but gathers itself into a cloud, which, after hovering for a time, settles itself over the places of maximum vibration."

In Savart's memoir, "Sur les Vibrations Normales" ("An. de Ch. et de Ph." for 1827, xxxvi, 187), the author distinctly claims the above-named discovery. At p. 190 he refers to the nodal lines of Chladni, but adds that by mixing with the sand a finer dust, such as lycopodium, "la poussière fine se réunit pour tracer d'autres lignes circulaires que ce physicien n'a pas connues," &c.

Faraday, in his critical examination of Savart's memoir ("Phil. Trans.," 1831, p. 299), apparently takes it for granted that Savart started with an original observation.

But this interesting discovery, which has been so fruitful in beautiful results, is really due to Chladni. In his "Traité d'Acoustique," Paris, 1809, he remarks, p. 125: "Si un peu de poussière fine est mêlée au sable, elle pourra mieux servir pour faire voir aussi les centres des vibrations, c'est-à-dire, les endroits où les parties vibrantes font les plus grandes excursions: les molécules les plus petites de la poussière s'accumuleront sur ces endroits."

Chladni is even more explicit in his "Neue Beyträge zur Akustic," (4to, Leipzig, 1817). At p. 7 he recommends "etwas *Pulvis lycopodii*" as the fine dust to be mixed with the sand; and at p. 69, he remarks that when fine dust accumulates on the centres of vibration, it is in heaps more or less round or long, &c., according to the form assumed by the vibrating part.

When Wheatstone reproduced Chladni's figures on square plates ("Phil. Trans.," 1833, p. 593), he did not notice the remarkable figures produced by mixing a fine powder with the sand. This was the less necessary because Faraday's memoir had been so recently published, and its conclusion was so satisfactory, namely, that when a plate is vibrating, currents are established in the air lying upon the surface of the plate, which pass from the nodal lines towards the centres of maximum vibration, and then proceeding outwards from the plate to a greater or less distance, return towards the nodal lines.

With the exception of a very few elementary specimens on a small scale, as given by Chladni and Faraday, I am not aware that this class of figures has ever been adequately represented; and yet these figures are not only interesting in themselves, but are capable of being exhibited on a scale fitted for the lecture table. For some of the figures we may make use of a brass plate, 12 inches square, fixed on a pillar by means of a central screw. The upper surface of the plate should be blackened by means of nitrate of silver or chloride of platinum solution, or some such contrivance, so as to leave a stain not liable to crack or peel off.

A thin layer of sand is to be dusted over the surface by means of a pepper-box, and then a small bag of thin linen or calico, containing the lycopodium powder, is to be shaken over the layer of sand. On applying the bow to the edge of the plate, $3\frac{1}{4}$ inches from the corner, the note produced in my plate is E♮ in the fourth space. This produces fig. 1 with well-defined nodal lines and heaps of lycopodium powder on the twelve centres of vibration. Under the continued action of the bow each heap expands, swells up, and contracts with great activity. If while this figure is at rest the bow be applied to the centre of the edge, so as to produce the higher note B♭ above the lines in the treble clef, fig. 2 is produced, a figure of great beauty, from the symmetrical arrangement of its parts and the delicate variations of shading, which confer upon it an artistic character.

Iron filings were sifted over the plate, and on sounding the note that leads to the formation of fig. 1, the finest particles of iron were transferred to the centres of vibration, while the coarser remained at the nodes.

Lycopodium alone was dusted over the plate, when the powder became divided into two portions, one remaining on the nodal lines, now very much broader than those formed by sand, while a second portion assumed active functions on the vibrating centres. Lord Justice Fry, F.R.S., to whom I showed this experiment, suggested that the winnowing process which separated the finer from the coarser particles in the former experiments might also apply here. He collected specimens from the nodes and from the vibrating centres, but

on examination found that "the spores from both are of the same size, all being, I believe, macrospores." I have since examined a number of commercial specimens of lycopodium, but have not met with any containing microspores.

The powder of sublimed sulphur was also dusted over the plate, when on sounding the note E, fig. 1 was beautifully produced, the nodal lines being well defined, and not much coarser than with sand; but the flowers on the vibrating centres were much more diffused, shading off into cloudiness much more than with lycopodium. The sulphur also was not so active. Figs. 3 and 4 are attempts to represent the action of sulphur alone.

After a layer of iron filings had been sifted on the plate, a layer of sand was added, when the finer particles of iron did not escape to the vibrating centres, they being apparently imprisoned by the sand.

Flowers of sulphur being dusted on the plate, and then lycopodium, a certain amount of winnowing goes on under the influence of vibration, the sulphur for the most part going to the nodes, and the lycopodium to the centres (fig. 5). When either of these or a similar light powder is used alone, the slight adhesion of the powder to the plate on and about the nodal lines is sufficient to retain it there, while the looser upper particles are swept upon the centres.

The cyclonic air currents are well shown by flowers of sulphur, when the plate is thrown into its greatest amplitude of vibration by sounding either of the two lowest notes, D in the bass clef, and A♭ below the lines of the treble clef, the figures being the well known crosses which terminate, one in the centre of the edges, and the other at the angles of the plate. In either case there are but four vibrating centres, and the powder occupies a considerable space on each, and is whirled round in numerous eddies, which can be distinctly followed by the eye, showing how the separate heaps of lycopodium are formed when this powder is used instead of sulphur, which does not collect in heaps, but in curved parallel ripples, convex towards the fixed centre of the plate.

The brass plate thus fixed yields but a limited number of figures. By using plates of various material and form, such as bell metal bronze, white metal, glass, &c., in squares, parallelograms, ovals, &c., fixed at any point by means of Chladni's clamp, an endless variety of forms may be obtained. In the following figures the small white circle shows where the plate was clamped.

The presence of flowers of sulphur gives a softened effect to the figure, and converts the sharp angles formed by sand alone into graceful curves. Fig. 6 shows the effect of sulphur and lycopodium combined; fig. 7 of sand and lycopodium, where the finer powder collects in heaps. In the more complicated figures which follow it is remarkable to note the precision with which the lycopodium becomes sepa-

rated from the sand, and performs its evolutions within the numerous centres of vibration into which the plate breaks up.

With respect to the double circles (figs. 18 and 19), and the double ellipse (fig. 16), Chladni remarks that these are the most difficult of all the figures to produce. With the two powders, sand and lycopodium, the difficulty ceases, the lighter powder forming the outer and the sand the inner ring.

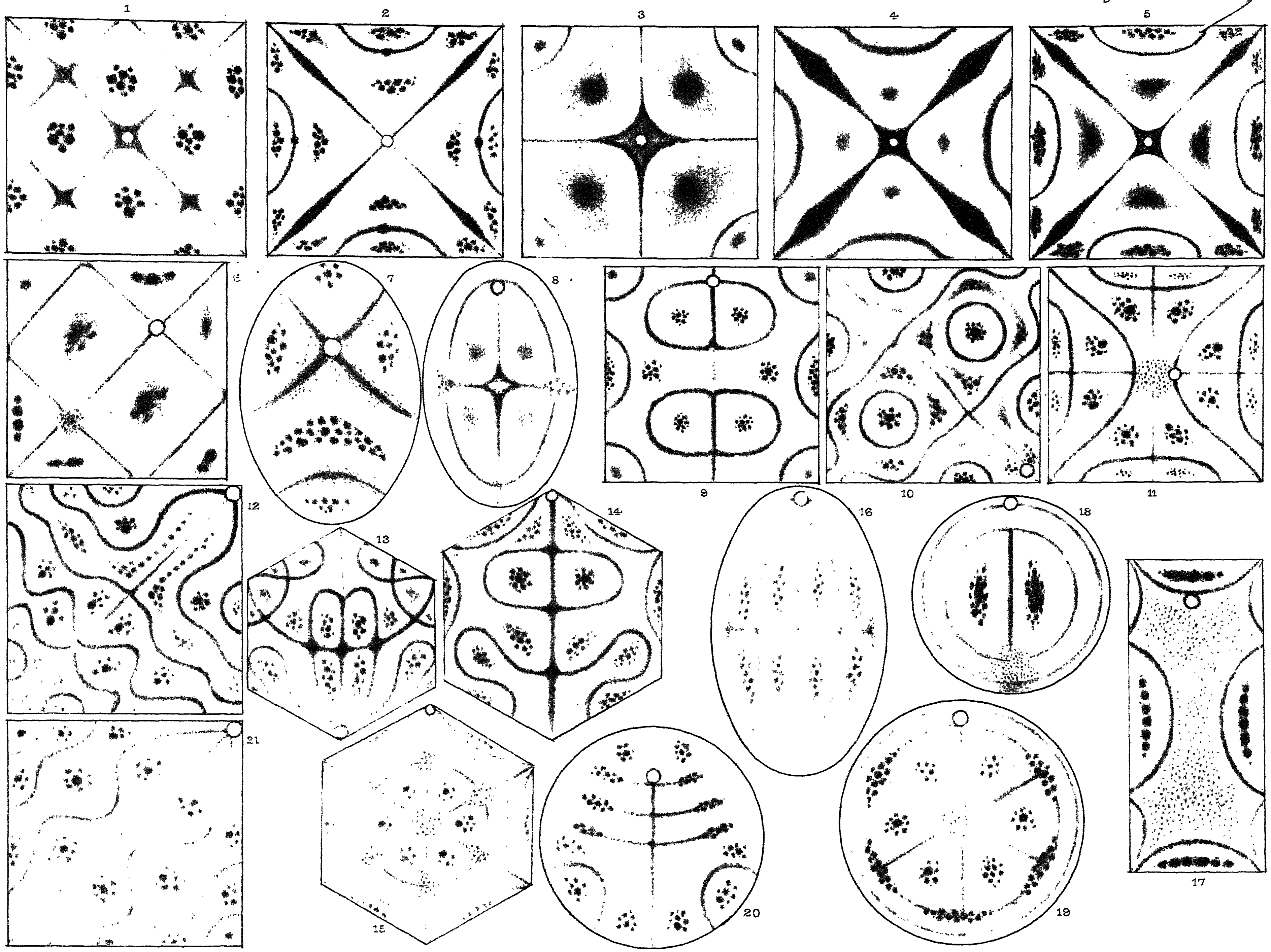
The figures represented have been faithfully copied from the plates; such distortions as those represented in fig. 10 are due to irregularities in the structure of the plate.

DESCRIPTION OF THE PLATE.

No. of Figure.	Material of the plate.	Dimensions.	Materials employed.
1.	Brass.	12 in. × 12 in.	Sand and lycopodium.
2.	"	"	" "
3.	"	"	Sulphur.
4.	"	"	" "
5.	"	"	Sulphur and lycopodium.
6.	Bronze.	8 in. × 8 in.	" "
7.	Brass.	{Major axis, 7 in. / Minor axis, 5 in.}	Sand and lycopodium.
8.	"	"	Sand and sulphur.
9.	White metal.	7 in. × 7 in.	Sand and lycopodium.
10.	Bell metal.	"	" "
11.	Brass.	"	" "
12.	"	8 in. × 8 in.	" "
13.	"	7 in. external diameter.	" "
14.	"	"	" "
15.	"	"	" "
16.	"	{Major axis, 7 in. / Minor axis, 5 in.}	" "
17.	"	8 in. × 4½ in.	" "
18.	Glass.	8 in. diameter.	" "
19.	"	9 in. diameter.	" "
20.	"	"	" "
21.	Bell metal.	7 in. × 7 in.	" "

NOTE.—When the plate is clamped near the edge, the bow should be applied as near to the clamp as may be convenient. If far removed therefrom, the amplitude of the vibrations is too great to produce a good figure, and glass plates are liable to be broken.

Tomlinson. Proc. Roy. Soc. Vol. 38. Pl. 1.



Mary Tomlinson del.t

West, Newman & Co. lith.

II. "On the General Characters of the Genus *Cymbulia*." By JOHN D. MACDONALD, M.D., F.R.S., Inspector-General R.N. Received March 21, 1885.

The purely pelagic habit of the interesting order of Pteropods places them so far beyond the reach of zoologists in general, that the opportunities of examining them in a living or recent state are few and far between. I have myself been fortunate enough to obtain, from time to time, nearly all the leading genera of the order in question, but, singularly, have never fallen in with *Cymbulia*.

Judging from the figures and descriptions of this genus given by naturalists, it always appeared to me as if both animal and shell were taken end for end so as to render all the descriptive relationships ambiguous; but it was only a short time ago that I was enabled to investigate the point practically through the kindness of a naval friend who brought me some specimens of *Cymbulia Peroni* from the Indian Ocean.

The result of examination proved the impression above expressed to be a correct one, while other particulars of interest also revealed themselves. I found that the attachment of the animal to the shell was so slight, and the visceral mass so short and rounded, that by incautious handling the animal very readily came away, and here I believe is the whole secret of the matter, for, on attempting to place it *in situ* again, one would be very likely to turn it upside down, and the error would, of course, be propagated in otherwise, possibly, excellent figures and descriptions. It might be mentioned here that the great malacologist De Blainville gave the generic name of *Gastroplax* to an *Umbrella* which he found with the broad surface of the foot accidentally adherent to the shell, so that we need not wonder at what anybody else might do under similar circumstances.

Mr. S. P. Woodward, in his Manual of Mollusca, thus characterises *Cymbulia*:—

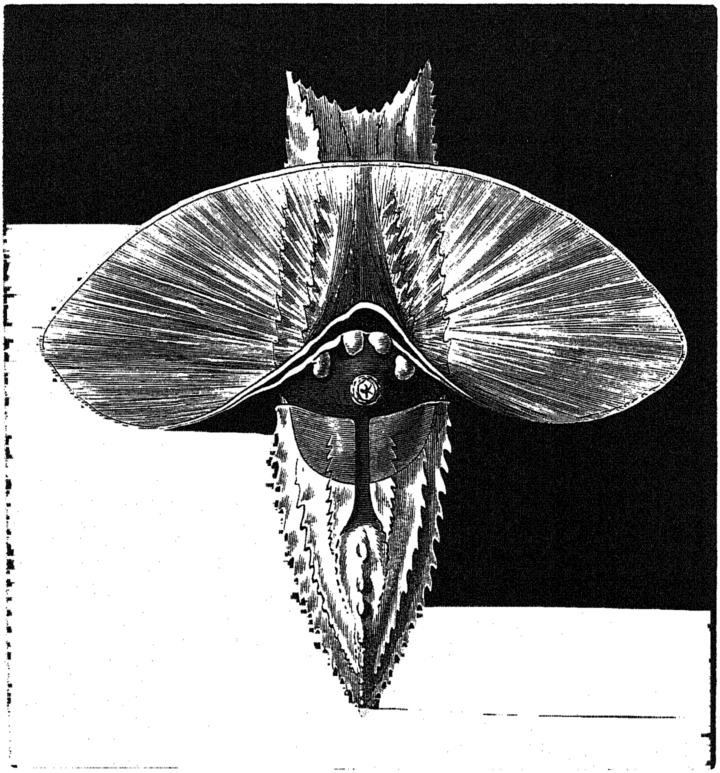
Shell cartilaginous, slipper-shaped, pointed *in front*, truncated *posteriorly*, aperture elongated ventral.

Animal with large rounded fins connected ventrally by an elongated lobe; mouth furnished with minute tentacles, lingual teeth, 1,1,1; stomach muscular, armed with two sharp plates.

In keeping with the above description, the figure given of *Cymbulia proboscidea* (after Adams) shows the toe of the slipper in front like a rostrum, probably suggesting the specific name *proboscidea*, for the animal itself presents no appearance of a proboscis. In De Blainville's figure, however, a protrusion from the generative orifice might be so interpreted; but unfortunately, though the shell is given in its natural position, the animal is turned upside down. The eyes men-

tioned by the same authority I have not been able to discover, but in the figure they are represented as set in the two central processes, which would appear to be a modification of the metopodium of other Pteropods.

The fins are said to be rounded and connected ventrally (?) by an elongated lobe, but this statement is not quite correct. The two fins



together form a broad continuous reniform plate, with an unbroken curved outline in front, and a hilum-like notch behind, where the mouth of the animal is situated. Nevertheless, the epipodial expansion exhibits a tendency to fall into folds that might suggest the idea of a ventral lobe if the animal were reversed in its shell.

The posterior margin of the fins on each side presents a dilamination which gradually widens towards the middle line so as to include the mouth, and form two distinct, but quite linear, labia or lips.

The beautifully transparent shell of this species (probably *C. Peroni*)

is about an inch in length, and a quarter of an inch across the instep, from which it tapers elliptically to a pointed toe, while the heel at the anterior extremity is abruptly truncated and broadly notched.

The slipper is ornamented with eight principal spiny ridges, taking a longitudinal direction, diverging from the toe and terminating separately at the heel. But the central vertical ridge corresponding with the front of the slipper is cut off where a square notch or fissure in the instep receives the muscular attachment of the animal. The whole scheme of arrangement will be better understood on inspecting the accompanying figure, which is about three times the natural size.

III. "On the Agency of Water in Volcanic Eruptions; with some Observations on the Thickness of the Earth's Crust from a Geological Point of View; and on the Primary Cause of Volcanic Action." By JOSEPH PRESTWICH, F.R.S., Professor of Geology in the University of Oxford. Received March 26, 1885.

(Abstract.)

That water plays an important part in volcanic eruptions is a well-established fact, but there is a difference of opinion as to whether it should be regarded as a primary or a secondary agent, and as to the time, place, and mode of its intervention. The author gives the opinions of Daubeny, Poulett-Scrope, and Mallet, and dismissing the first and last as not meeting the views of geologists, proceeds to examine the grounds of Scrope's hypothesis — the one generally accepted in this country—which holds that the rise of lava in a volcanic vent is occasioned by the expansion of volumes of high pressure steam generated in the interior of a mass of liquefied and heated mineral matter within or beneath the eruptive orifice, or that volcanic eruptions are to be attributed to the escape of high pressure steam existing in the interior of the earth. The way in which the water is introduced and where, is not explained, but as the expulsion of the lava is considered to be due to the force of the imprisoned vapour, it is, of course, necessary that it should extend to the very base of the volcanic foci, just as it is necessary that the powder must be in the breech of the gun to effect the expulsion of the ball.

The author then proceeds to state his objections to this hypothesis. In the first place, he questions whether it is possible for water to penetrate to a heated or molten magma underlying the solid crust. The stratigraphical difficulties are not insurmountable, although it is well known that the quantity of water within the depths actually

reached in mines, decreases, as a rule, with the depth, and is less in the Palæozoic than in the Mesozoic and Kainozoic strata.

The main difficulty is thermo-dynamical. As the elastic vapour of water increases with the rise of temperature, and faster at high than at low temperatures, the pressure,—which at a depth of about 7,500 feet and with a temperature (taking the thermometric gradient at 48 feet per 1° F.) of 212° F., would be equal to that of one atmosphere only,—would at a depth of 15,000 feet and a temperature of 362°, be equal to $10\frac{1}{2}$ atmospheres, and at 20,000 feet and temperature of 467°, would exceed 25 atmospheres. Beyond this temperature the pressure has only been determined by empirical formulæ, which, as the increase of pressure is nearly proportional to the fifth power of the excess of temperature, would show that the pressure, in presence of the heat at greater depths, becomes excessive. Thus, if the formulæ hold good to the critical point of water or 773°, there would at that temperature be a pressure of about 350 atmospheres.

At temperatures exceeding 1000° F. and depth of about 50,000 feet, the experiments of M. H. St. Claire Deville have shown that the vapour of water, under certain conditions, probably undergoes dissociation, and, consequently, a large increase in volume. It would follow also on this that if the water-vapour had been subject to the long-continued action of the high temperatures of great depths, we might expect to meet with a less amount of steam and a larger proportion of its constituent gases than occurs in the eruptions. Capillarity will assist the descent, and pressure will cause the water to retain its fluidity to considerable depths, but with the increasing heat, capillarity loses its power.

Taking these various conditions into consideration, the author doubts whether the surface waters can penetrate to depths of more than seven to eight miles, and feels it impossible to accept any hypothesis based upon an assumed percolation to unlimited depths. That there should be open fissures through which water could penetrate to the volcanic foci, he also considers an impossibility.

But the objection to which the author attaches most weight against the extravasation of the lava being due to the presence of vapour in the volcanic foci, is, that if such were the case, there should be a distinct relation between the discharge of the lava and of the vapour, whereas the result of an examination of a number of well-recorded eruptions, shows that the two operations are in no relation and are perfectly independent. Sometimes there has been a large discharge of lava and little or no escape of steam, and at other times there have been paroxysmal explosive eruptions with little discharge of lava.

There are instances in which the lava of Vesuvius has welled out almost with the tranquillity of a water-spring. A great eruption of Etna commenced with violent explosions and ejection of scoriæ,

which after sixteen days ceased, but the flow of lava continued for four months without further explosions. In the eruption of Santorin of 1866, the rock-emission proceeded for days in silence, the protruded mass of lava forming a hill nearly 500 feet long by 200 feet high, which a witness compared with the steady and uninterrupted growth of a soap bubble. The eruptions of Mauna Loa are remarkable for their magnitude and at the same time for their quiet. Speaking of the eruptions of 1855, Dana says there was no earthquake, no internal thunderings, and no premonitions. A vent or fissure was formed, from which a vast body of liquid lava flowed rapidly but quietly, and without steam explosions, for the space of many months.

On the other hand, paroxysmal eruptions are generally accompanied by earthquakes, and begin with one powerful burst, followed rapidly by a succession of explosions, and commonly with little extrusion of lava, although it is to be observed that a large quantity must be blown into scoriæ and lost in the ejections. Such was the eruption of Coseguina in 1835, and of Krakatoa in 1883. Sometimes in these paroxysmal eruptions, there is absolutely no escape of lava, scoriæ alone being projected. A common feature in eruptions, and which indicates the termination of the crisis, is the stopping of the lava, though the gaseous explosions continue for some time with scarcely diminished energy.

There is, thus, no definite relation between the quantity of explosive gases and vapours and the quantity of lava. If the eruption of lava depended on the occluded vapour, it is not easy to see how there could be great flows without a large escape of vapour, or large volumes of vapour without lava. The extrusion of lava has been compared to the boiling over of a viscid substance in a vessel, but the cases are not analogous.

The only logical way in which it would seem possible for water to be present, is on the hypothesis of Sterry Hunt, who supposes the molten magma to be a re-melted mass of the earlier sedimentary strata, which had been originally subject to surface and meteoric action. But in the end the preceding objections apply equally to this view.

There is the further general objection to the presence of water in the molten magma, in that were the extrusion of lava due to this cause, the extrusion of granite and other molten rocks (which do not as a rule lie so deep as the lava magma) should have been the first to feel its influence and to show its presence. Yet although water is present, it is in such small quantities, that these rocks never exhibit the scoriaceous character which lava so commonly possesses.

Nor is lava always scoriaceous, as it should be if the hypothesis were correct. Many lavas are perfectly compact and free from vapour

cavities, and so also are especially most of the great sheets of lava (basalt), which welled out through fissures in late geological times. These vast fissure eruptions, which in India and America cover thousands of square miles, and are several thousand feet thick, seem conclusive against water agency, for they have welled out evidently in a state of great fluidity, with extremely little explosive accompaniments, and often without a trace of scoriæ mounds. The general presence of non-hydrated rocks and minerals is also incompatible with the permeation of water which the assumption involves.

It has been suggested by some writers that large subterranean cavities may exist at depths in which the vapour of water is stored under high pressure, but the author shows that such natural cavities are highly improbable in any rocks, and impossible in calcareous strata.

The author proceeds to account for the presence of the enormous quantity of the vapour of water, so constantly present in eruptions, and which, in one eruption of Etna, was estimated by Fouqué to be equal to about 5,000,000 gallons in the twenty-four hours. He refers it to the surface-waters gaining access *during the eruptions* to the volcanic ducts either in the volcanic mountain itself, or at comparatively moderate depths beneath. He describes how the springs and wells are influenced by volcanic outbursts. By some observers, these effects have been referred to the influence of dry and wet seasons, but there are so many recorded instances by competent witnesses, as to leave little doubt of the fact. This was also the decision of the inquiry by the late Professor Phillips, who asks, why is the drying up of the wells and springs an indication of coming disaster?

The author then considers the hydro-geological condition of the underground waters. He points to the well-known fact, that on the surface of volcanoes the whole of the rainfall disappears at once, and shows that when the mountain is at rest, the underground water must behave as in ordinary sedimentary strata. Therefore, the water will remain stored in the body of the mountain, in the interstices of the rocks and scoriæ, and in the many empty lava-tunnels and cavities. The level of this water will rise with the height of the mountain, and he estimates that it has at times reached in Etna a height of 5,000 to 6,000 feet, while the permanent level of the springs at the base of the mountain seems to be at about 2,000 feet. The water does not, however, form one common reservoir, but is divided into a number of independent levels by the irregular distribution of the scoriæ, lava, &c. These beds are traversed by vertical dykes running radially from the crater, so that, as they generally admit of the passage of water, the dykes serve as conduits to carry the water to the central duct.

Little is known of the sedimentary strata on which volcanoes stand.

In Naples, however, an artesian well found them under the volcanic materials in usual succession, and with several water-bearing beds, from one of which, at a depth of 1,524 feet, a spring of water rose to the surface with a discharge of 440 gallons per minute. When in a state of rest, the surplus underground waters escape in the ordinary way by springs on the surface, or when the strata crop out in the sea, they then form submarine springs.

During an eruption, these conditions are completely changed. The ascending lava, as it crashes through the solid plug formed during a lengthened period of repose, comes in contact with the water lodged around or may be in the duct, which is at once flashed into steam, and gives rise to explosions more or less violent. These explosions rend the mountain, and fresh fissures are formed which further serve to carry the water to the duct from which they proceed; or they may serve as channels for the sea-water to flood the crater, when, as in the case of Coseguina and Krakatoa, the volcano is near the sea-level. As the eruption continues, the water stores immediately around the duct become exhausted, and then the water lodged in the more distant parts of the mountain rushes in to supply the void, and the explosions are violent and prolonged according to the available volume of water in the volcanic beds. When this store is exhausted, the same process will go on with the underlying water-bearing sedimentary strata traversed by the volcanic duct.

The author gives diagrams showing the position of the water-levels *before*, *during*, and *after* eruption; and describes the manner in which, if the strata surrounding the duct and below the sea-level become exhausted, the efflux of the fresh water which passed out to sea through the permeable beds, when the inland waters stood at their normal height above the sea-level, these same beds will in their turn serve as channels for the sea water to restore the lowered water-level inland. Thus, the excurrent channels which carried the land waters into the sea-bed, and there formed, as they often do off the coasts of the Mediterranean, powerful fresh-water springs, now serve as channels for an incurrent stream of sea water, which like the fresh waters it replaces, passes into the volcanic duct. This agrees with the fact that diatomaceous fresh-water remains are common in many eruptions, and marine remains in others; also, that the products of decomposition of sea water are so abundant during and at the close of eruptions. With the fall of the water-levels, the available supply of water becomes exhausted, or the channels of communication impeded, and this continues until, with the ceasing of the extravasation of the lava, the eruption comes to an end.

The author then explains the way in which the water may gain access to the lava in the duct, notwithstanding heat and pressure.

This he considers to be dependent upon the difference between the statical and the kinetical pressure of the column of lava on the sides of the duct. In the change from the one state to the other, when the lava begins to flow, and its lateral pressure is lessened, the equilibrium with the surrounding elastic high pressure vapour becomes destroyed, and the vapour forces its way into the ascending lava. As this proceeds, the heated water further from the duct, and held back by the pressure of the vapour, flashes into steam to supply its place. If that water should be lodged in the joints of the surrounding rock, blocks of it will also be blown off, driven into, and ejected with, the ascending lava, as have been the blocks in Somma and of other volcanoes.

It is the double action thus established between the inland- and sea-waters that has probably prolonged the activity of the existing volcanoes settled in ocean centres, or along coast lines, while the great inland volcanic areas of Auvergne, the Eifel, Central Asia, &c., have become dormant or extinct.

But if water only plays a secondary part in volcanic eruptions, to what is the motive power which causes the extravasation of the lava to be attributed? This involves questions connected with the solidity of the globe far more hypothetical and difficult of proof. The author first takes into consideration the probable thickness of the earth's crust from a geological point of view, and shows, that although the present stability of the earth's surface renders it evident that the hypothesis of a thin crust resting on a fluid nucleus is untenable, it is equally difficult to reconcile certain geological phenomena with a globe solid throughout, or even with a very thick crust. The geological phenomena on which he relies in proof of a crust of small thickness, are:—1. Its flexibility as exhibited down to the most recent mountain uplifts, and in the elevation of continental areas. 2. The increase of temperature with depth. 3. The volcanic phenomena of the present day, and the out-welling of the vast sheets of trappean rocks during late geological periods.

He considers that the squeezing and doubling up of the strata in mountain chains—as, for example, the 200 miles of originally horizontal strata in the Alps, crushed into a space of 130 miles (and in some cases the compression is still greater)—can only be accounted for on the assumption of a thin crust resting on a yielding substratum, for the strata have bent as only a free surface plate could to the deformation caused by lateral pressure. If the globe were solid, or the crust of great thickness, there would have been *crushing* and *fracture*, but not *corrugations*. Looking at the dimensions of these folds, it is evident also that the plate could not be of any great thickness. This in connexion with the increase of heat with depth, and the rise of the molten lava through volcanic ducts, which, if too

long, would allow the lava to consolidate, leads the author to believe that the outer solid crust may be less even than 20 miles thick.

That the crust does possess great mobility is shown by the fact that since the glacial period, there have been movements of continental upheaval—to at least the extent of 1,500 to 1,800 feet—that within more recent times they have extended to the height of 300 to 400 feet or more, and they have not yet entirely ceased.

With regard to the suggestion of the late Professor Hopkins that the lava lies in molten lakes at various depths beneath the surface, the author finds it difficult to conceive their isolation as separate and independent local igneous centres, in presence of the large areas occupied by modern and by recently extinct volcanoes. But the chief objection is, that if such lakes existed they would tend to depletion, and as they could not be replenished from surrounding areas, the surface above would cave in and become depressed, whereas areas of volcanic activity are usually areas of elevation, and the great basaltic out-wellings of Colorado and Utah, instead of being accompanied by depression, form tracts raised 5,000 to 12,000 feet above the sea-level.

These slow secular upheavals and depressions, this domed elevation of great volcanic areas, the author thinks most compatible with the movement of a thin crust on a slowly yielding viscid body or layer, also of no great thickness, and wrapping round a solid nucleus. The viscid magma is thus compressed between the two solids, and while yielding in places to compression, it, as a consequence of its narrow limits, expands in like proportion in conterminous areas. As an example, he instances the imposing slow movements of elevation which have so long been going on along almost all the land bordering the shores of the Polar Seas, and to the areas of depression which so often further south subtend the upheaved districts.

With respect to the primary cause of these changes and of the extravasation of lava, the author sees no hypothesis which meets all the conditions of the case so well as the old hypothesis of secular refrigeration and contraction of a heated globe with a solid crust,—not as originally held, with a fluid nucleus, but with the modifications which he has named, and with a *quasi rigidity* compatible with the conclusions of the eminent physicists who have investigated this part of the problem. Although the loss of terrestrial heat by radiation is now exceedingly small, so also is the contraction needed for the quantity of lava ejected. Cordier long since calculated that supposing five volcanic eruptions to take place annually, it would require a century to shorten the radius of the earth to the extent of 1 mm. or about $\frac{1}{25}$ inch.

The author, therefore, concludes that while the extravasation of the lava is due to the latter cause, the presence of vapour is due alone to

the surface and underground waters with which it comes into contact as it rises through the volcanic duct, the violence of the eruption being in exact proportion to the quantity which so gains access.

IV. "On the Fibrin-yielding Constituents of the Blood Plasma." By L. C. WOOLDRIDGE, M.B., D.Sc., Demonstrator of Physiology in Guy's Hospital. From the Laboratory of the Brown Institution. Communicated by Professor MICHAEL FOSTER, Sec. R.S. Received March 26, 1885.

There is no doubt that from every variety of blood plasma a proteid body may be isolated, which can by appropriate means be converted into fibrin. This body, which is known as fibrinogen, has been more especially studied by Hammarsten. This observer has shown that fibrinogen possesses characters which clearly distinguish it from the other supposed factor in coagulation, viz., paraglobulin, and also that solutions of fibrinogen will, when treated with fibrin ferment, give rise to fibrin. The only objection possible to Hammarsten's experiments is that the body which he isolated has either previously to or during the process of isolation undergone alteration. That it is in fact not the same body which is present in the circulating blood, but that it is, so to say, a sort of nascent fibrin. My observations bear on this point.

Peptone plasma is obtained by injecting a solution of peptone into the veins of an animal, and bleeding it directly afterwards. The blood does not clot, and by means of the centrifuge the plasma is obtained. The injection of peptone produces this effect by preventing the interaction of leucocytes and plasma which normally takes place in shed blood.* By repeated centrifugalising, the whole of the corpuscular elements can be removed from this plasma, and the pure plasma thus obtained can be made to clot in the most complete manner, giving rise to a large quantity of fibrin, and this without the addition of any further proteid body, so that the plasma must contain dissolved in it the mother substance or substances of fibrin.

In a note presented to the Society a few weeks ago, I described a new constituent of the plasma which gives rise to fibrin and to other bodies concerned in coagulation. I need not refer at length in the present paper to this new substance. It is separable from the plasma by cooling the latter, and after its removal the plasma still yields a large quantity of fibrin, and from this plasma, by Hammersten's method, a body can be isolated, agreeing in all particulars with Hammersten's fibrinogen, and clotting readily with fibrin ferment.

* Wooldridge: "Zur Gerinnung des Blutes," "Archiv für Physiol.," Jahrg. 1883, p. 389.

The following observations refer to such a plasma in which the peptonisation is very complete, and from which the body separable by cooling has been removed.

*Behaviour of the Plasma towards Fibrin Ferment and Serum.**

In the vast majority of cases, the plasma gives with either of the above only a very minimal clot, a few scarcely perceptible threads or membranes being the sole result of prolonged action. Serum is not more effectual than ferment.

But if, after the addition of serum or ferment, a stream of carbonic acid be passed through the plasma, or the plasma be diluted with several times its volume of water, it clots through and through, becoming quite solid.

The readiness with which coagulation takes place with CO_2 varies in different specimens, sometimes very rapidly, sometimes more slowly; sometimes it only occurs when the CO_2 treatment and dilution are combined.

There are two exceptions to the above statement:—

Firstly—Sometimes neither ferment nor serum give by themselves the slightest trace of a clot in the plasma.

Secondly—They sometimes give a very considerable clot.

Both these exceptions are rare.

Now, I take these experiments to show that the plasma contains a certain very small amount of true fibrinogen (coagulable with serum). In some cases even this small trace may be absent, in others it may be considerable. But the bulk of the coagulable matter of the plasma is not directly coagulable with serum. It is a body which is readily altered so as to clot with the serum or ferment. This alteration can be effected by dilution, by CO_2, or in the process of isolation. The body is not fibrinogen, but it readily passes into the latter.

I now turn to another kind of plasma which gives like results, but which is free from the objection that in peptone plasma the proteids become altered by the peptone injection—an objection which I do not think to be at all justified by the facts. For the sake of convenience, I call it NaCl plasma. It is obtained by receiving blood direct from the artery into a 10 per cent. solution of common salt, equal quantities of blood and salt solution being taken.

It is essential that the blood should be mixed with the salt solution with as little delay as possible. Very frequently the plasma obtained from this blood is a little stained with hæmoglobin.

Now if to this plasma ferment be added, a certain amount of clotting rapidly takes place; it is usually very inconsiderable. On

* Serum from dog's blood. Ferment prepared from serum (dog's) according to Schmidt's method. The dry powder is added directly to the plasma, to avoid the effect of dilution.

very long standing no increase takes place. If, however, after the removal of the slight clot, the plasma be diluted with four times its volume of water, it clots through and through.

Now 4 or 5 per cent. solution of salt does not interfere with the action of the fibrin ferment, and hence we must conclude that as in peptone plasma, so in salt plasma, the bulk of the coagulable matter is not in the form of fibrinogen, but as a substance which must first be altered by dilution.

These conclusions are confirmed by the behaviour of the plasma on heating. As is well known, solutions of Hammersten's fibrinogen coagulate on heating to 54—56° C.

If some NaCl plasma which has been treated with ferment, and from which the slight clot thereby caused has been removed, be heated to 56°, it remains perfectly clear; long exposure to this temperature does not alter it, and it can be heated up to a very high temperature, 90° and upwards, without the slightest coagulum forming, though at high temperatures it becomes opalescent.

The exact upper limit of coagulation varies; it is usually over 90° C. Of course if hæmoglobin be present, it interferes with the experiment. Now it will be remembered that this plasma, in spite of the removal of the small quantity of fibrin, contains a large quantity of fibrin-yielding matter.

The fact that ferment gives a slight clot in NaCl plasma may be taken as an indication that NaCl plasma does contain a certain small amount of true fibrinogen, and in fact if NaCl plasma which has not been treated with ferment be heated to 56°, it becomes turbid, and a slight coagulum forms. This, like the clot obtained by ferment, is sometimes very small indeed, sometimes more considerable.

Before describing the behaviour of peptone plasma on heating, I must make a slight digression.

In my note previously referred to,* I showed that there exists dissolved in the plasma a body separable by cooling. So long as this body is present, coagulation is produced by passage of a stream of carbonic acid through the plasma, the whole of the coagulable substance being converted into fibrin. At the same time not only is fibrin ferment produced, but also a body capable of converting the coagulable body of the plasma into fibrinogen. To make this perfectly clear, I shall give an example.

Peptone plasma, rich in the substance separable by cold, is treated with CO_2; it coagulates, the serum is pressed out from the clot and allowed to stand, when it again becomes alkaline. On adding some of this serum to a new portion of plasma, the latter clots completely and with great rapidity. The clotting is much more rapid than the original clotting with CO_2.

* "On a New Constituent, &c.," *ante*, p. 69.

Now this serum contains fibrin ferment; but inasmuch as ferment does not cause anything more than a trace of clot in peptone plasma, the serum must evidently contain some special body which renders the plasma coagulable by ferment. It must contain the precursor of the fibrinogen which, as we have seen, appears to exist dissolved in plasma. I do not know what this special substance is; it is derived from the body separable by cold and also from leucocytes. I will remark that it is not contained in ordinary serum, and hence is not paraglobulin.

Peptone plasma behaves in a totally different manner on heating, according as it contains a considerable amount of the body separable by cold or not. If this body be present, the plasma remains, on heating to 56—57°, perfectly clear for a short time—five to fifteen minutes; it then becomes gradually turbid, and finally a dense flocculent precipitate forms; but it is a very long time—hours—before this precipitate reaches its maximum.* If it be absent, the plasma does not give on prolonged heating to 56—57° any coagulum, and remains perfectly free from any precipitate till a very high temperature, 80—90°, is reached. It becomes opalescent at high temperatures. In either case, whether the body separable by cold be absent or present, the plasma, if mixed with an equal quantity of 10 per cent. salt solution, remains free from any precipitate till 80—90°, or higher, is reached. The upper limit varies somewhat, often being as high as 95°.

It must be understood that the precipitate mentioned above as occurring in plasma at 56—57° may or may not contain the body separable by cold, but it appears in very much larger quantity there than does the latter, and represents the whole of the coagulable matter of the plasma. It would therefore appear that in the process of heating, the same special body is liberated from the substance separable by cold, as is the case when the latter is acted on by carbonic acid, *i.e.*, a body capable of converting the precursor of fibrinogen into fibrinogen precipitable at 56°, and coagulable with fibrin ferment. The presence of 5 per cent. NaCl prevents its development by means of carbonic acid, and also by means of heat.

In order not to obscure the point I have just been discussing, I have left out of consideration the very small trace of true fibrinogen pre-existent in peptone plasma. This was mentioned at the beginning of the paper; it always separates on heating the plasma to 56°, whether it has been cooled or not, or whether salt be present or not, but it is generally so very small in quantity that it is difficult to see the minute flocculi that separate at 56°; hence the statements I made above as to the plasma remaining clear are practically correct. It must not however be forgotten that in some rare cases, peptone plasma

* This slow clotting at 56–57° was observed by Fano.

does give a considerable clot with serum or ferment alone, and such a plasma gives a dense clot at 56°, whether the substance removable by cold be present or not.

Now it is a well known fact that the injection into the veins of a strong solution of fibrin ferment, prepared in the ordinary way, is very rarely followed by any serious thrombosis. The same is true of defibrinated blood, and this is quite in conformity with the results I have described above.

This note is only a slight addition to the results I have already obtained in the coagulation question, and I am actively engaged in pursuing the subject.

I wish to take this opportunity of making a few statements with regard to the action of lecithin in producing coagulation. I have already shown that lecithin is an important factor in the coagulation of the blood. Since these publications,* my observations have only tended to completely confirm the statements I have already made, and I have in addition found that lecithin from the most varied sources, and lecithin prepared from the platinum salt, is perfectly active. The sources which have yielded an active lecithin are lymph glands, blood, testis, brain, yeast. Moreover, I have found that it exerts its influence on other varieties of plasma besides those which I have already quoted, viz., peptone plasma and cooled plasma. As to its exact mode of action I am not yet certain. It no doubt gives rise to the appearance of a large quantity of fibrin ferment, as was described in my note "On the Origin of the Fibrin Ferment," but I have reason to think that it has a further action.

The majority of the experiments on which the above paper has been founded, were carried out at the laboratory of the Brown Institution.

* "Further Observations on the Coagulation of the Blood," "Journal of Physiol.," 1883; "On the Coagulation of the Blood," "Journal of Physiol.," 1884; "On the Origin of the Fibrin Ferment," "Proc. Roy. Soc.," 1884.

April 23, 1885.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Magnetisation of Iron." By JOHN HOPKINSON, M.A., D.Sc., F.R.S. Received March 30, 1885.

(Abstract.)

The paper contains an account of the results of experiments which have been made on a considerable number of samples of iron and steel of known composition, including samples of cast iron, malleable cast iron, wrought iron, ordinary steels, manganese, chromium, tungsten, and silicon steels. The electrical resistance and the magnetic properties are determined in absolute measure. Amongst the electrical resistances the most noteworthy fact is the very high resistance of cast iron, as much as ten times that of wrought iron. The fact that manganese steel is almost non-magnetic is verified, and its actual permeability measured. The action of manganese appears to be to reduce the maximum magnetisation of steel, and in a still greater ratio the residual magnetism, but not to affect the coercive force materially. It is shown that the observed permeability of manganese steel containing 12 per cent. of manganese would be accounted for by assuming that this alloy consists of a perfectly non-magnetic material, in which are scattered about one-tenth part of isolated particles of pure iron. Some practical applications of the results are discussed.

II. "On the Changes produced by Magnetisation in the Length of Rods of Iron, Steel and Nickel." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Professor GUTHRIE, F.R.S. Received April 1, 1885.

(Abstract.)

The earliest systematic experiments on the effects produced by magnetisation upon the length of iron and steel bars are those of

Joule, an account of which is published in the "Phil. Mag." of 1847. Joule's experiments have many times been repeated, and his general results confirmed. In particular, Prof. A. M. Mayer carried out a series of very careful observations with apparatus of elaborate construction and great delicacy.* The conclusions at which he arrived were in accord with those of Joule, so far as regards iron: in the case of steel there was some apparent discrepancy, which, however, might to a great extent be accounted for by differences in the quality of the metal used, and in the manner of conducting the experiments. In 1882 Prof. Barrett published in "Nature" an account of some experiments which he had made, not only on iron but also on bars of nickel and cobalt, with the view of ascertaining the effect of magnetisation upon their length.

The knowledge on the subject up to the present time may be summarised as follows :—

1. Magnetisation causes in iron bars an elongation, the amount of which varies up to a certain point as the square of the magnetising force. When the saturation point is approached the elongation is less than this law would require. The effect is greater in proportion to the softness of the metal.

2. When a rod or wire of iron is stretched by a weight, the elongating effect of magnetisation is diminished; and if the ratio of the weight to the section of the wire exceeds a certain limit, magnetisation causes retraction instead of elongation.

3. Soft steel behaves like iron, but the elongation for a given magnetising force is smaller (Joule). Hard steel is slightly elongated, both when the magnetising current is made and when it is interrupted, provided that the strength of the successive currents is gradually increased (Joule). The first application of the magnetising force causes elongation of a steel bar if it is tempered blue, and retraction if it is tempered yellow: subsequent applications of the *same* external magnetising force cause temporary retraction, whether the temper of the steel is blue or yellow (Mayer).

4. The length of a nickel bar is diminished by magnetisation, the maximum retraction being twice as great as the maximum elongation of iron (Barrett).

In order that the results of Joule and Mayer might be comparable with those obtained by the author, he made an attempt to estimate the magnetising forces with which they worked. From data contained in their papers, it was calculated that the strongest magnetising force used by Joule was about 126 units, while the strongest used by Mayer did not on the highest probable estimate exceed 118 units. In the author's experiments the magnetising force was carried up to about 312 units. The metal rods, too, were much smaller than any

* "Phil. Mag.," 1873, vol. xlvi, p. 177.

which had been before used for the purpose, ranging in diameter from 1·40 to 6·25 mm. Their length was in every case 100 mm., and the apparatus was capable of measuring with tolerable certainty an elongation or retraction equal to a ten-millionth part of this length.

By using thinner iron rods and greater magnetising forces than those previously employed, the following curious and interesting fact was established. If the magnetisation be carried beyond a certain critical point the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing with the magnetising force. If the force is sufficiently increased, a point is arrived at where the original length of the rod is totally unaffected by magnetisation; and if the magnetisation be carried still further, the original length of the rod will be reduced. It also appeared that the position of the critical point in steel depended in a very remarkable manner upon the hardness or temper of the metal; considerable light is thus thrown on the apparently anomalous results obtained by Joule and by Mayer. Further experiments disclosed strong reason for believing that the value of the critical magnetising force in a thin iron rod was greatly reduced by stretching; this would explain the fact that Joule obtained opposite effects with stretched and unstretched wires.

By ascertaining the relative values of the temporary moments induced by gradually increasing external magnetising forces, an attempt was made to connect the point of maximum elongation with a definite phase of the magnetisation of the several rods in which the elongation had been observed.

Though more experiments must be made before it is possible to generalise from them with perfect safety, the results so far obtained by the author indicate the laws given below. The elongations and magnetisations referred to are temporary only; before the beginning of an experiment the rod was permanently magnetised by passing through the magnetising coil a current equal to the strongest subsequently used. In iron the greatest elongation due to permanent magnetisation was generally found to be about one-third of the total elongation, while in nickel the permanent retraction amounted only to about one twenty-fifth part of the whole.

I. *Iron.*

1. The length of an iron rod is increased by magnetisation up to a certain critical value of the magnetising force, when a maximum elongation is reached.

2. If the critical value of the magnetising force is exceeded, the elongation is diminished until with a sufficiently powerful magnetising force the original length of the rod is unaffected, and if the force is

still further increased the rod undergoes retraction. Shortly after the critical point is passed, the elongation diminishes in proportion as the magnetising force increases. The greatest actual retraction hitherto observed was equal to about half the maximum elongation, but there was no indication of a limit, and a stronger magnetising force would have produced further retraction.

3. The value of the external magnetising force corresponding to maximum elongation is for a given rod, approximately equal to twice its value at the "turning point."

Definition.—The turning point in the magnetisation of an iron bar is reached when the temporary moment begins to increase less rapidly than the external magnetising force.

4. The external force corresponding to the point of maximum elongation increases (when the quality of the iron is the same) with the diameter of the rod. So also does its value at the turning point.

5. The amount of the maximum elongation appears to vary inversely as the square root of the diameter of the rod, when the quality of the iron is the same.

6. The turning point, and therefore presumably the point of maximum elongation, occurs with a smaller magnetising force when the rod is stretched than when it is unstretched.

II. *Steel.*

7. In soft steel magnetisation produces elongation, which, as in the case of iron, increases up to a certain value of the magnetising force, and afterwards diminishes. The maximum elongation is less than in iron, and the rate of diminution after the maximum is passed is also less.

8. The critical value of the magnetising force for a steel rod diminishes with increasing hardness up to a certain point, corresponding to a yellow temper; after which it increases, and with very hard steel becomes very high. There is therefore a critical degree of hardness for which the critical magnetising force is a minimum; in steel of a yellow temper the value of the critical magnetising force is lower than in steel which is either softer or harder.

9. In soft steel a strong magnetising force subsequently diminished may cause a greater temporary elongation than the diminished force is capable of producing if applied in the first place.

10. A temporary elongation when once produced in soft steel may be maintained by a magnetising force which is itself too small to originate any perceptible elongation.

III. *Nickel.*

11. Nickel continues to retract with magnetising forces far ex-

ceeding those which produce the maximum elongation of iron. The greatest observed retraction of nickel is more than three times the maximum observed elongation of iron, and the limit has not yet been reached.

12. A nickel wire stretched by a weight undergoes retraction when magnetised.

III. "The Essential Nature of the Colouring of Phytophagous Larvæ (and their Pupæ); with an Account of some Experiments upon the Relation between the Colour of such Larvæ and that of their Food-plants." By EDWARD B. POULTON, M.A., of Jesus and Keble Colleges, Oxford. Communicated by Professor J. S. BURDON SANDERSON, F.R.S. Received April 11, 1885.

CONTENTS.

1. *The Essential Nature of the Colouring of Phytophagous Larvæ.*

Phytophagous larvæ owe their colour and markings to two causes: (1) Pigments derived from their food-plants,* chlorophyll and xanthophyll, and probably others; (2) pigments proper to the larvæ, or larval tissues made use of because of some (merely incidental) aid which they lend to the colouring, *e.g.*, fat. A larva may be coloured by either or both of these groups of factors. It may be generally stated that all green colouration without exception, as far as I have investigated the subject, is due to chlorophyll; while nearly all yellows are due to xanthophyll. All other colours (including black and white) and some yellows, especially those with an orange tinge, are due to the second class of causes (as far as I am aware: it is, however, extremely probable that certain colours will be proved to arise from the modification of the derived pigments; and many observations make it probable that other colours may be derived from plants in the case of larvæ feeding upon the petals, &c.). The derived pigments often occur dissolved in the blood, or segregated in the subcuticular tissues (probably the hypodermis cells), or even in a chitinous layer, closely associated with the cuticle itself. This last situation has only as yet been proved in the case of the pupa of *Papilio Machaon*, but it is almost certainly true of many other pupæ. The colours proper to the larvæ occur in the hypodermis cells and in the cuticle. The commonest structural basis of variability or polymorphism in larvæ is afforded by the varying extent to which either of these factors takes part in producing colouration. Thus brown and green are by far the commonest instances of dimorphism, and when this is the case the former is due to larval pigment, the latter to derived plant pigments. It is very remarkable that there should be such an essential difference between the larvæ from the same batch of eggs, as far as the causes of colour are concerned. Mr. Raphael Meldola has very conclusively argued for chlorophyll as the cause of the colour in green larvæ in a paper in the "Proc. Zool. Soc." for

* It will be shown that the derived chlorophyll of larvæ has undergone chemical changes of which the effects appear in the altered spectrum, and in the great stability of the pigment when present in the animal blood or tissues. If, on the other hand, the chlorophyll be separated from the proteid with which it is united in the animal, it is so unstable that the colour disappears at once, and it is impossible to obtain it in solution by the use of the ordinary solvents of vegetal chlorophyll (or by any reagents of which I have made use). The spectrum and chemical characters are very constant, seeming to prove that the same definite changes are wrought by many species of insect. At the suggestion of Professor E. Ray Lankester, I propose to call this derived pigment *metachlorophyll*. The spectrum of the green blood of the pupa of *Ephyra Punctaria* differs from that of other larvæ and pupæ hitherto examined, and I propose for this form of derived pigment the term *Ephyra-chlorophyll*. There is not at present sufficient ground for giving a distinctive name to the derived xanthophyll.—May 4th, 1885.

1873, p. 155. He explains the relation between larval and vegetal colouration, by supposing that the larvæ have been rendered transparent by natural selection, and that thus the vegetal colours are themselves seen and give the colour to the larvæ. He points out that internal feeders are not green (their food being without chlorophyll), and he quotes Chautard (" Compt. rend.," January 13, 1873) as stating that he had discovered chlorophyll in the tissues of insects.

Mr. Meldola further expands this theory in his editorial notes to his translation of Weismann's " Studies in the Theory of Descent," Part II, " On the Origin of the Markings of Caterpillars," &c. Here he adduces the important evidence that " some few species of Nepticula (*N. Oxyacanthella*, *N. Viscerella*, &c.) are green, although they live in leaf-galleries where this colour can hardly be of use as a protection, but their food (hawthorn and elm) contains chlorophyll." He also quotes Chautard (" Ann. Chim. Phys." [5], iii, 1—56) in addition to the previous reference to the same author, stating on this authority that chlorophyll is a substance of great chemical stability, and that it resists the animal digestive processes. On referring to Chautard, I find that Mr. Meldola is mistaken in the statement that Chautard had discovered chlorophyll in the tissues of insects. In both papers referred to Chautard expressly states that the chlorophyll came from the digestive tract of the insects (Cantharides) experimented upon, and that he failed to obtain the bands after macerating the elytra in alcohol. He indeed states that chlorophyll resists the digestive processes, meaning that it is detected unchanged in the fæces. In a note to one of the papers (" Ann. Chim. Phys.") Chautard does not seem to be entirely satisfied with his proof of the absence of chlorophyll in the elytra of cantharides. He states that if present the chlorophyll must be much altered, and the colour of the elytra cannot be due to it, and he points out that the colouring matter is very insoluble in alcohol and ether. He alludes to Pocklington's researches (recently quoted by Dr. MacMunn), but only mentions and disputes the possibility of ascertaining the plant upon which the cantharides fed by a spectroscopic examination of an extract of these insects.

Dr. MacMunn in a paper on Animal Chromatology in the " Proceedings of the Birmingham Philosophical Society" (1882—3, vol. iii, Pt. II, p. 351), states that all previous attempts to find chlorophyll in the colouring of insects have led to negative results. (There is one partial exception to this in the case of Chautard's hesitation about chlorophyll in the elytra of cantharides, and there is also Pocklington's work on the same subject mentioned by Dr. MacMunn in a later paper.) He states that by concentrating light on the integument of the larva of *Pieris Rapæ* and examining with a micro-spectroscope, he found a band in the red which showed a resemblance to chlorophyll.

In the reports of the British Association at Southport (1883), Dr. MacMunn further states that on withdrawing the intestinal contents, the green colour and band disappear (Krukenberg also states that the band is due to the contents of the digestive tract, quoted in a letter to "Nature" by Dr. MacMunn for the week ending January 10th, 1885). I have not yet examined the larva of *P. Rapæ* from this point of view, but I think it is very probable that the green colour is due to the blood which may have been also lost when the contents of the intestine were removed. I shall show later on that chlorophyll (or some modification of it) passes through the digestive tract and tinges the blood of Lepidopterous larvæ, and that the colour of green larvæ is often due to this cause alone. I am sure that the contents of the digestive tract cannot account for the ground colour of a larva (such as this), in which the green is at all bright and distinct. Furthermore, the blood is extremely likely to be lost in any manipulation of a larva, for it exists under pressure and exudes from a very small aperture. If the intestine was dissected out, there can be doubt of the loss of the blood.

Dr. MacMunn also alludes to the discovery of chlorophyll in the elytra of cantharides, and states that he has confirmed this observation. He believes that this chlorophyll is a synthetic production, and suggests that it may be used for protective purposes, but that probably a pigment of less complicated chemical constitution might answer as well. Of course this would not apply to the derived (and modified) chlorophyll of Phytophagous larvæ.

Dr. MacMunn's interesting discovery of entero-chlorophyll (which he believes to be synthetically formed) in the liver of many invertebrates, does not apply to the present question. (See "Proc. Roy. Soc.," vol. 35, No. 226, p. 370.)

2. *Methods by which the two Factors of Larval Colouration may be Recognised: Instances of their Occurrence.*

If the body of a small individual of the green form of a dimorphic (green and brown) Noctua larva be gently compressed, the green tint disappears from this part, and is replaced by faint yellow. The markings of the larva (apart from the ground colour) are unchanged. At the same time the tracheal system and other internal structures become easily visible. Conversely, the swollen uncompressed part of the body becomes of a far deeper green than it was before. This is especially well seen in the true legs, of which the colour is normally pale. Such effects are due to the fact that the green colour depends upon the green blood. (I use this word for the more correct term, hæmolymph.) This conclusion is confirmed by examining the blood itself, and by observing that the green ground colour of the larva dis-

appears on removing the bright green blood. If, on the other hand, a brown variety of the same larva be compressed, the results are much less marked, and chiefly dependent upon the difference in transparency of the compressed and swollen parts of the larva. The blood is generally found to be colourless or very faintly yellowish: hence the ground colour of the green form depends upon its blood, while that of the brown form depends upon its proper pigment. Intermediate colours are produced by intermediate conditions, and it is quite common to find a series connecting the green and brown larvæ. On compressing a greenish-brown larva, the green at once becomes apparent in those parts of the organism which are deficient in pigment, *i.e.*, the under surface and claspers; and indeed it may be generally stated that the colour due to the blood generally predominates in the lower part of the body, while that due to larval pigment usually predominates on the back. The most extreme brown varieties have often lost all colour in their blood (except when it is seen in a very thick layer). *Phlogophora Meticulosa* is a common species upon which these observations can be made. Not only are these two factors so variably developed in dimorphic or polymorphic larvæ, but the same variability is very commonly seen in a single life-history. Thus many green larvæ become brown after one of the ecdyses, or brown ones become green. Hence in the period preceding ecdysis the blood must have lost its colour (*P. Meticulosa* and many Noctuæ), or the larval pigment must have become so opaque as to obscure any more deeply placed colour (common among Geometers). There are often great advantages to be gained by a brown larva retaining the green colour in its blood. This is well seen in the case of *Ennomos Angularia*, of which the larva is an opaque Geometer, resembling the dark twigs of the elm. The pupal period is very short (in the middle of the summer), and the cocoon is consequently very slight. It is formed of a few leaves loosely bound together, between which the larva and afterwards the pupa, are easily seen. Here therefore the brown colour is inappropriate, and the whole of the larval pigment is discharged, allowing the green blood to give its colour to the larva during the quiescent period before pupation. The pupa is similarly coloured and is also green. (It is dimorphic, with two shades of green. These facts about *E. Angularia* have been communicated to the Entomological Society in a hitherto unpublished paper, but the explanation has not appeared before.) Furthermore, these larvæ, when very young, live upon the surface of the leaf they are eating, and at this time they are coloured green by their blood, the pigment having not yet been deposited in a sufficient amount to render the surface opaque. Hence the alternation of colours, which follows the corresponding succession of objects imitated, depends upon the shifting scenes resulting from the presence of green blood behind a superficial covering, which can be rendered

transparent and opaque alternately. Even in the most opaque Geometer larvæ, exactly resembling the dark twigs of their food-plants, the inner surfaces of the large posterior claspers are often green or yellowish. This is the one spot in the body which is always hidden, and here only is the pigment absent and the tint of the blood visible.

In some cases the green colour of the blood disappears, and is replaced by a tint which helps towards producing the brown colouration. Thus the larva of *Chærocampa Elpenor* is at first green, becoming brown at the beginning of the fifth stage (as a rule). Examining the blood of a pupa of this species, I found that it was of a reddish-brown colour, and must therefore have helped in causing the ground colour of the last stages. Although no traces of the green colour were to be seen, the spectra of modified chlorophyll and of xanthophyll were distinct in a considerable thickness of the fluid. Evidently the larva had not destroyed the pigments of the previous stages beyond the point at which they ceased to interfere with the effect of the later stage. The brownish tint in the blood of this brown larva plays a far less important part in the total colouration than the green colour of the blood in green larvæ, as is proved by the following considerations:—It is not uncommon to find green larvæ of *C. Elpenor* in the last stage, and it may be assumed that the blood has then retained the green tint. But even in these green larvæ there is a considerable amount of dark pigment in and near the four eye-like terrifying marks on the first and second abdominal segments, and the brown colour (where it occurs) is not obviously different from that of a normal brown variety. Again, in blown specimens of *Chærocampa Porcellus* the brown colour remains distinct and dark, although the blood has, of course, been removed. And yet, considering the very complete resemblances between the larvæ of *C. Elpenor* and *C. Porcellus* throughout their life-history, it is practically certain that the blood of the latter is similar in colour to that of the former.

I will now give instances of the segregation of the derived pigments in the subcuticular tissues (probably the hypodermis cells). This must be true of *Smerinthus Ocellatus*, inasmuch as this larva remains green after dissection when the blood has escaped. Again, the green ground colour of the larvæ of *Sphinx Ligustri*, *S. Ocellatus*, &c., turns brown on the dorsal surface before pupation, but is comparatively unchanged elsewhere, while the green blood is also unchanged in the pupa. This latter fact is also true of the pupa of *Smerinthus Tiliæ*, although the adult larva becomes purplish-brown without a trace of green remaining (even upon the paler under-surface). Hence these larvæ must be coloured by superficial segregation of the derived pigments probably strengthened by the colour of the

blood. On the other hand there are larvæ which entirely depend upon the green blood for their colour, and there is a transparency about such larvæ which at once distinguishes them from those above described. Thus green Noctua larvæ are usually of this kind. The species of Noctua which exhibits the most typically transparent green (that I know of) is the larva of *Gonoptera Libatrix* (although I have never investigated the constitution of the colouring in this species). Again, pressure has a comparatively slight effect upon the distribution of colour in larvæ of the former division, while it produces the greatest effect in those of the latter. I think it is very probable that in many cases the derived pigments may only exist in the blood in the early stages, and may be deposited later in the superficial tissues. Such an arrangement would entirely accord with a very common change in the appearance of green larvæ during growth. There is generally a marked difference in the colour of a green larva after its first meal, and before this the transparency is so complete that its tracheæ and other internal organs can usually be seen. But it does not therefore follow that the chlorophyll is at this time deposited in the superficial tissues (although such may be the case), for the facts may be accounted for by an increase in the amount of colour in the blood, and by a change in its tint (the probability of this will be shown presently).

3. *The Different Results of Methods of Preservation upon the two Factors in Larval Colouration.*

The derived pigment can always be distinguished from that proper to the organism, in larvæ which have been preserved in spirit, or as blown and dried specimens. After both processes the proper pigment is unchanged, being quite insoluble in spirit and uninjured by drying. The derived pigments, on the other hand, dissolve in the spirit from the blood and from the superficial tissues, while in blown larvæ the blood is removed (with the other contents of the body), and the chlorophyll in the superficial tissues rapidly fades, while the yellow due to the xanthophyll may persist for a long time. Looking over a dried collection, it is at once obvious that green larvæ, such as *P. Rapæ*, *S. Populi*, *S. Tiliæ*, *Notodonta Camelina*, &c., have entirely changed in colour, while others, such as *Liparis Auriflua*, *Bombyx Neustria*, *Odonestis Potatoria*, *Eriogaster Lanestris*, &c., remain perfectly life-like. Hairy, brightly-coloured larvæ are generally unchanged, as also are those distasteful forms which are protected by conspicuous colours (such as *Abraxas Grossulariata*, *Euchelia Jacobeæ*, &c.). Smooth brown larvæ (such as the majority of Geometers and many Noctuas) have evidently lost some depth of colour, due to the removal of the contents of the body and the con-

sequent transparency; but the changes are slight compared with those shown by green larvæ, and are evidently due to the hypodermis pigment being in amount or in character insufficient to render the surface entirely opaque. In the instances mentioned above as being identical in appearance with the living larvæ the pigment is entirely opaque. In such larvæ some of the pigment is often deposited in the cuticle, and is therefore visible in the latter after being thrown off at ecdysis. As a rule the cast off cuticle is transparent and colourless, except the last one thrown off at pupation, which is generally discoloured and brown, because it is moistened by the fluid which covers the pupa, and which hardens and darkens into the protective layer of the latter. The effect in spirit specimens is even more striking than in the other method of preservation, for the green colour is not merely changed, but has entirely disappeared, leaving the surfaces colourless. This is very obvious with larval colouration into which both factors prominently enter. Thus in *Dicranura Vinula* (kept for a year and a half in spirit) the green ground colour has entirely faded, and the spirit is yellow with dissolved xanthophyll, while the large purple area on the back with its white marginal line is entirely normal.

4. *Colours due to Larval Tissues independently of Pigment.*

There is one sentence in Dr. Weismann's essay (on the markings of caterpillars) which shows that he attributes importance to the deeply placed substances in producing larval markings. Speaking of the second stage of the larva of *Macroglossa Stellatarum* (p. 246, English translation), he says:—"A dark green dorsal line, which, however, does not arise from the deposition of pigment, as is generally the case, but from a division in the folds of the fatty tissue along this position." According to my experience the dorsal line is generally formed in this way when its colour is similar to that of the ground colour, except for a deeper tint. This division in the folds of fat is occupied by the dorsal vessel, and the latter can be distinctly seen contracting; its margins being thus shown to be coextensive with the dorsal line, which is therefore really due to the derived pigment in the blood. (This is true of *S. Ocellatus*, *Sphinx Ligustri*, &c.) But the superficial colours can be caused by an accumulation of internal tissues immediately beneath the transparent skin quite independently of pigment. Weismann implies in the above-quoted sentence that the fat exercises a modifying influence upon colour. But it may do more than this; it may be the only factor in certain markings. I have already described ("Trans. Ent. Soc.," April, 1884) a Phytophagous Hymenopterous larva *Nematus* (*Curtispina*) in which the dark-green dorsal line was due to the blood of the dorsal vessel, while on each side of this line there was

a broad white band. Examination proved that the white bands moved with the contractions of the dorsal vessel, and showed the characteristic structure of fatty tissue. Dissection and the use of the microscope confirmed these conclusions. The rest of the larva was green (due to its blood), with a slight black shading below the white bands. The shading was the only part of the colour due to larval pigment. The skin was so transparent that the tracheal system was quite distinct, and the fat was removed from all the conspicuous surfaces (except in the region of the bands) through which it would have been visible, and would have added other white markings. But this tissue was collected superficially along the median ventral line, producing a ventral white band, which was always hidden, for the larva rests with its ventral surface applied to the edge of a leaf.

Last autumn (1884) I found that fat can also serve as the vehicle for other colours. I was working at another of the Phytophagous Hymenoptera—the larva of *Crœsus Septentrionalis*, and I found that the yellow colour round the lips of the seven median ventral eversible glands is due to fat collected at these points, and the yellow colour of the posterior segments also depends upon the same cause, the colour being contained in the fat globules themselves.

It is extremely likely that fat will prove to be a more important factor in larval colouring than has been hitherto supposed.

5. *Changes in the Two Factors of Larval Colouration before Pupation.*

It has already been mentioned that the superficial derived pigments of *S. Ligustri* become brown in the dorsal region, before pupation, while the colour of the blood is unchanged. In *D. Vinula* the whole larva becomes reddish-brown, and in this case the green blood changes to brownish-yellow. The true larval pigment also changes before pupation, except when it is cuticular. Thus the larva of *E. Angularia* becomes transparent by the disappearance of dark pigment, and the green blood gives its colour to the larva. The green colour of the blood is generally retained in the pupal state, and it is often of great importance, as will be shown presently. The changes in colour before pupation alluded to above, are for the protection of the larvæ in their new surroundings, against which their old colouration would be very conspicuous. And this is therefore only true of a limited number of larvæ which wander in search of a place in which to pupate, or are exposed against a new background (as *E. Angularia*). In all larvæ, however (except those entirely coloured by cuticular pigment), the colours ultimately fade in the quiescent and contracted state immediately preceding pupation. Even in green Noctua larvæ coloured by their blood, the fat is no longer withdrawn from the surface, and the deep green tint can only be seen in the interstices of this lobulated tissue.

In some cases there are indications of quite new markings in the changes preceding pupation. Thus in the larva of *G. Libatrix* black patches appear on the sides of two (I believe) of the anterior segments, while green patches appear on corresponding parts of the larva of *Selenia Illunaria.*

6. *The Causes of Colour in Pupæ.*

In the pupæ of nearly all Heterocera (except the few that are protectively coloured like the larvæ, *e.g.*, *Ephyridæ*, *E. Angularia*, &c.), the larval colours have entirely disappeared from the surface, and are replaced by the darkened opaque cuticle of the pupa. The derived pigments are, however, generally retained in the blood. Before the covering has darkened, immediately after the last ecdysis, the pupa possesses a colour partly due to its blood (when coloured), and partly to its fat body. The effect of the former is seen in the parts where the latter is absent, especially in the wings, legs, and antennæ. These parts are therefore green or yellow in the pupæ of which the blood is coloured by derived pigments. The rest of the body is white, and through the transparent cuticle the lobulated fat body can be distinctly seen. These appearances can only be witnessed for a very short time, since the semi-fluid surface rapidly darkens into the normal reddish-brown or black; although it is not uncommon for the colour of the blood to be seen through the cuticle of the wings—the most transparent part. There are, however, instances in which some of the true larval markings can be traced upon the pupa directly after emergence from the larval skin. Thus in *S. Ligustri* the oblique stripes are distinctly seen upon their respective segments. In a specimen placed in spirit before the appearances were obscured, the oblique stripes have darkened into brown, while the surrounding surface remains comparatively pale. This subject will repay future investigation, and I think it is probable that larval markings will prove to be commonly present upon the pupæ, if examined sufficiently early. When the pupæ of Heterocera are freely exposed, the constitution of their protective colouring is similar to that of the larvæ. Thus in the dimorphic (green or brown) species of Ephyra larva, the colour of the pupa follows that of the larva. The green is due to chlorophyll in the blood, and the colour is especially deep over the wings and adjacent surfaces. The depth of colour is produced in certain positions by reflection from a white surface of fat behind a layer of green blood, the light having thus traversed the fluid twice; while in other positions the pupa is transparent. There are also black markings in the Ephyra pupa, which are due to cuticular pigment, and therefore remain in the empty pupa case.

The pupa of *A. Grossulariata* is believed to be gaudily coloured

(black and yellow) as a signal of distastefulness, and the appearance is entirely due to cuticular pigment, and persists in the empty shell. The pupæ of Rhopalocera are nearly all exposed, and protectively coloured. Most of the colours are due to the above-described causes, derived or proper pigment; but in some cases they cannot be the result of pigment at all, and have not yet been sufficiently investigated (*e.g.*, the metallic colours of Vanessa pupæ). I have worked at the colour in the pupa of *P. Machaon*, which appears in two chief forms connected by intermediate varieties, the forms being coloured by derived and proper pigments respectively. Those coloured by derived pigments are yellow or green and yellow, the green being predominant in the anterior part of the body, especially the wings, &c., but also occurring in minute lines and masses mixed with the yellow. The other form is grey with dark markings. The blood is always bright yellow, with contained xanthophyll, but this does not aid the superficial colour, as the cuticle is opaque. The derived pigments are contained in a remarkable and very complicated layer beneath the true cuticle. This layer is very finely lamellated, but at the same time possesses a prismatic structure, being composed of columns arranged vertically to the surface, and themselves made up of parallel fibrillæ. It is cuticularised, and stains deeply in picric acid, but it is sharply marked off by a fine homogeneous membrane from the true cuticle above, and the subcuticular tissues below. The whole structure needs further investigation, but I believe that the layer is a cuticularised and very complex result of the differentiation of the hypodermis cells. The pigments are contained in the outer lamellæ, while the inner remain white. Although the layer is very firm and completely united to the true cuticle (which it resembles in consistence), neither itself nor its contents are beyond the reach of the physiological processes of the organism, for the pigments are withdrawn shortly before the emergence of the imago, and the wings of the latter are visible through the now transparent covering. At the same time the whole inner layer must itself disappear, for the empty pupa cases of this species are thin, and evidently composed of the true cuticle only, as in other pupæ; while the inner layer makes the whole pupal covering very thick, being itself many times thicker than the true cuticle. I have no doubt that this layer will be found commonly in Rhopalocera, and it is probably correlated with the extreme development of protective colours in the pupæ of this group. The pupal pigments upon the dark varieties, and in some parts of most of the green varieties, are placed in the true cuticle.

7. *The Ultimate Use of the Derived Pigments.*

In many species there is a great positive gain in retaining the derived pigments in the blood (or other part) of the pupa, for by their

means the ova are coloured green or yellow, and the pigments are passed down into the next generation. In this way the newly hatched larva is tinged, and the break in protective colouring is thus filled up, which would otherwise occur before the results of the first meal could become manifest. Hence it might be supposed that the pigments would be retained in the case of female pupæ laying green eggs, but not necessarily always in the case of males. Nevertheless the pigments appear to be retained equally in both sexes, the only partial exceptions (if any exist) being in the case of dimorphic larvæ (green and brown) of which the females are more generally green and the males brown, and I do not think that there are any authentic instances of such a division. In Newman's "Moths" it is stated of *C. Elpenor* "caterpillar green or brown, the males generally brown," but I have never heard of any confirmation of this statement, and furthermore, the existence of a brown female larva would have to be explained. Last autumn the only larva I could obtain of this species was brown, with brown blood in the pupa, and it proved to be a male. It may, however, be considered certain that the females are sometimes brown, for all collectors seem to admit that the brown variety is far commoner than the green. It would be well to investigate the subject from this point of view. But I have already stated that the derived pigments are present in the brown blood of this species—in fact the characteristic spectrum of green blood was faintly but distinctly recognisable. Thus it is possible that there is sufficient pigment to tinge the ova, if collected in a comparatively small compass. Another possibility is that the pigments may undergo a slight change rendering them colourless, and that they are reconstructed for the ova. Against this hypothesis it must be urged that some of the pigment certainly remains unchanged. With the exception of this unsettled question my observations point uniformly in the same direction. I have examined the blood in the pupæ of the following species which lay green eggs :—*Smerinthus Ocellatus, Populi*, and *Tiliæ, Sphinx Ligustri.* In all cases the blood is green. *E. Angularia* is also another instance, the larva before pupation, the pupa itself, and the eggs being greenish.

On the other hand, *D. Vinula* lays large eggs, but they are reddish-brown from a deposit in the chitinous covering, and resemble spots on the leaves—to the upper sides of which they are affixed—and the young larvæ are black and are protected in the same way. In this species the blood of the pupa has lost the green colour which it possessed in the larva, becoming brownish-yellow.

It is easy to extract xanthophyll in alcoholic solution from ova in the bodies of female moths which have been preserved as cabinet specimens for many years. The eggs, when tinged by derived pigments, seem to owe most of their colour to xanthophyll, and they are

generally of a very yellowish-green. In the case of *E. Angularia* the eggs present (on a microscopic examination) a yellowish ground colour, with lines and patches of green. The pigments, at any rate partially, are dissolved in oil globules. The ova are opaque, and are coloured by reflected light, although no doubt a certain depth of substance is penetrated. In the same manner the newly hatched larva is always yellower than one which has taken a meal, apparently due to the predominance of xanthophyll in the former. In some cases there seems to be a segregation of the small amount of chlorophyll to a special part. Thus the head of the newly hatched *S. Ligustri* is green, while the rest of the body is yellow. These facts of the predominance of xanthophyll in the ovum and young larva are no doubt to be explained by the great stability of this derived pigment. Due to the same cause, this substance can be extracted from dried and blown larvæ that have been kept for many years, while the chlorophyll has entirely disappeared. It is probable that the green or yellow colour of the ovum is protective, especially considering that large and therefore easily seen ova are most generally tinted in this way, and because they seem to be deposited on the side of the leaf against which they would be least conspicuous (as I have noticed in the case of *S. Ligustri*). But the chief use of the pigments must be for the young larva, which often rests for some time before feeding, which is active, and therefore more easily recognised, and which does not possess a firm resistent covering like that of the ovum.

8. *The Probable History of Larval Colouration.*

It seems probable that food in the digestive tract was the first cause of larval colour. In root-feeding and other colourless larvæ the dark contents of the digestive tract are distinctly seen through the transparent varieties, and in many transparent coloured larvæ the same cause certainly helps to confer depth of colour. It is also probable that a dark dorsal line, due to the fluid contents of the dorsal vessel, was a very early marking, as also must have been the external effect of superficially placed fat. The next step would probably be the passage of the plant pigments through the digestive tract into the blood, and next from the blood into the subcuticular tissues, finally into so remarkable a tissue as that shown in *P. Machaon*. The employment of true pigment seems to be on the whole posterior in date to the use of derived pigment, and the former at first appears on the highly protected surfaces, the back and upper parts of the sides, conferring a distinct advantage in its greater number of available tints. In opaque larvæ, such as many brown Geometers, the green tints are still seen on the inner surfaces of the large posterior claspers where it was unnecessary to replace the older method of colouration. But these

stages were by no means mutually exclusive, or even necessarily successive, for each new method was an additional resource, not often to replace the rest, but in nearly all cases to be used with them. Hence the one of the commonest associations is a ground tint partially due to derived pigments in the blood, and also sometimes segregated beneath the cuticle, while special markings are conferred by true pigments generally placed in the hypodermis cells, although sometimes cuticular in position. The derived pigments more often confer general resemblances, the true pigments, special resemblances. Again the association is well seen in the use made of fat as a reflecting surface behind the transparent green blood. The fact that the colour due to derived pigments is almost incidental at first is shown by the possession of green blood by certain leaf-mining genera entirely concealed from view, as is pointed out by Mr. Meldola.

We can now summarise the causes of colour in larvæ with some attempt at the historic order of their employment:—

I. The internal tissues and organs with ready made colour......	*a.* Digestive tract. *b.* Fat. *c.* Dorsal vessel.
II. The passage of derived pigments through the walls of the digestive tract into	*a.* The blood. *b.* The subcuticular tissues.
III. The appearance of true pigment in	*a.* The hypodermis. *b.* The cuticle.

The above causes will, I believe, explain all larval and pupal colours, except such cases as the metallic tints of certain pupæ which cannot be entirely due to coloured substances.

9. *Spectroscopic Examination of the Blood of Lepidopterous Larvæ and Pupæ.*

In the following investigation Zeiss's microspectroscope was always employed, and proved to be extremely delicate and convenient on all occasions. The instrument was very kindly lent to me by Professor Burdon Sanderson. A paraffin lamp was at first used as the means of illumination, and it acted very well for the less refrangible half of the spectrum, but in all later work bright sunlight was alone employed because of its immense superiority at the violet end.

The blood was always obtained in such a way as to prevent admixture with the contents of the digestive tract or any secretions. Success in this attempt is aided by the fact that the blood exists under considerable pressure, so that after a minute prick, only just penetrating the hypodermis, a considerable quantity at once issues. In larvæ the distal parts of the claspers were generally pricked, the spot being chosen because of its situation at some distance from the diges-

tive tract. If the digestive tract had accidentally broken, fragments of the food would have been found in the blood when examined under the microscope, whereas it was always perfectly clear, containing only colourless corpuscles, fat-cells, and minute spherules of fat, which gradually came to the surface. Pupæ were generally pricked through the cuticle of the wings, and here also a considerable quantity of blood emerged under pressure. The whole of the blood was obtained by pushing the abdominal segments inwards, and ultimately by gradually increasing compression of the pupa. Owing to histolytic changes, the weak and thin-walled digestive tract is broken, and a red fluid escapes, which is mixed with the last of the blood. By carefully watching for the first appearance of the red fluid, the blood may be obtained in a perfectly pure state, exactly resembling that of the larva in clearness and in microscopic contents. A considerable quantity of gas also emerges usually just before the appearance of the fluid contents of the digestive tract, from which the gas also probably comes. The blood is received into sections of glass tubes of various lengths, with the ends carefully ground. One end is cemented with Dammar varnish to a glass slide, and when the tube is filled with blood a cover-glass is placed upon the open end, and becomes fixed by the drying of the blood. In most cases the blood so prepared will keep for months without change. If, however, air be admitted, an opaque black clot is formed on the surface, and the rest of the blood becomes cloudy. It will also keep, I believe, indefinitely in sealed tubes.

The Spectrum of Unaltered Blood.—(*a.*) *Larvæ of* P. Meticulosa.—These larvæ were very suitable for investigation, as all varieties of colour between green and brown are common. The blood of a green larva was taken up (December 12th, 1884) by a capillary tube with an internal diameter of about ·75 mm. The blood was allowed to stand four days, during which it evaporated to about half its bulk, but did not otherwise change; the tube was then sealed up. The colour was bright green, becoming darker after concentration. The spectrum was carefully noted (December 29th, 1884), a paraffin lamp being used. There was a broad band in the red, of which the extreme edges extended from 64·5—68·5, and when this band was best seen the violet end was cut off at 51, and the green was darkened to 52. There was no absorption of the red end. When the blood was fresh (December 12th, 1884) and less concentrated, the blue came through on the violet side of the darkening at 51, thus showing a broad dark band between this part and the green. A more concentrated sample of similar blood, prepared in the same way and at the same time, gave a darker band in the red with the same limits but with more defined edges. The violet end was similarly absorbed. There were indistinct traces of a broad dim band about 59—61·5.

The two tubes superposed gave a more marked spectrum, but the need of stronger light was much felt. The dark band broadened, extending towards the red end to 69·5, and past 64 towards the violet, dimming the red, and apparently uniting with the dim band at 60. The edge of this latter was nevertheless slightly darker than the dimmed red. The absorption of the violet end was unchanged.

The fresh blood of a larva of the same species, which had been bright green (due to its blood), but which after ecdysis became brown, was examined (December 31st, 1884) in a wide capillary tube about 1 mm. in internal diameter. The blood was almost colourless, *very* faintly yellow, and produced no effect on the spectrum.

At the same time the fresh blood of another individual of the same species, which was dark greenish-brown, due to a combination of subcuticular pigment and green blood, was examined in a capillary tube.

The compound character of the larval colouring was proved by gentle pressure. The pale green blood with a thickness of about 1 mm. gave the band in the red from 65—68, the violet end, being completely absorbed at 45, darkened to 51 (when the slit was narrowed so as to render the band distinct). A greater thickness of blood darkened the band, and cut off the violet end at 50, darkening to 52 (when the band was distinct). A still greater thickness produced more marked results with nearly the same limits. On widening the slit no blue appeared at the absorbed end. The dark band now seemed to extend to 68·5. The whole spectrum was much dimmed, but this was probably due to the accidental presence of fat in the blood. In this case the thickness of the fluid was 3·3 mm., and the colour was bright green.

The fresh blood of another individual of the same species, which was of a mahogany-brown colour, due to subcuticular pigment, was examined at the same time. When the larva was gently compressed, it became slightly yellowish-green in the swollen uncompressed part. A thickness of 4·5 mm. was faintly yellowish.

The spectrum showed a *very* faint band in the red, and the violet end was cut off as before, only less sharply and completely. Thus the latter character is more delicate as a test than the former. Hence in this considerable thickness there was far less effect than that produced by the blood of the last individual in a capillary tube. After being exposed to the air for $2\frac{1}{2}$ hours the blood became brown, but there was no difference in the spectrum.

Another brown individual of the same species was examined at the same time. When gently compressed it emitted a clear green fluid from the mouth, exactly resembling the blood of a green individual in its colour and transparency when examined in a thickness of 2·25 mm. (except that this was a little darker).

The spectrum showed the band in the red from 65·5—68·5, while the blue was cut off at 50, darkened to 52. The band was very sharp and distinct.

The blood of this individual was also examined, and it was of the same colour as the last, and possessed the same spectroscopic characteristics. This observation upon the green fluid from this digestive tract is important, because it serves to identify the chlorophyll in the blood with that taken in as food. It is likely, however, that a greater thickness of fluid and the use of sunlight would bring out some differences between the derived pigment dissolved in the digestive secretions, and that united with a proteid in the blood. It is also noteworthy that the chlorophyll was present in solution.

All the observations hitherto recorded were made with the illumination of a paraffin lamp. Early in January I found how much more could be seen in these spectra at the violet end by the use of bright sunlight. About the middle of January the more concentrated of the two specimens of green blood in sealed tubes was examined by sunlight, and the spectrum mapped (see Chart, Spectrum 1). The band in the red, reaching from 64·5—68, was very black, except at the edges. When this band was most distinct and clear the violet end was absorbed to 51, darkened to 52. On opening the slit a little, the blue came through (though dimmed) at 48, the violet end being absorbed at 43. When the slit was very narrow, traces of another band, from 59·5—61·5, were faintly seen. The spectrum was unchanged at the end of March, although the blood had been exposed to light for a considerable time. The two chief bands and the absorption of the violet end were also seen in the blood of a living larva by passing the light through one of the claspers.

(*b*.) *The Pupa of* Pygæra Bucephalus.—The blood of this species is of a very bright and beautiful green colour. I have examined it on a great many occasions, with all conditions of daylight. In the Chart, Spectrum 3, is shown the spectrum of the thickness of 23·5 mm. examined with bright sunlight. The characteristic band in the red ends sharply at 71, gradually at about 64·5, passing into a lesser absorption of the red, which is continuous with the second band, extending from about 58—60·5, but with very indistinct limits. When these appearances are best seen the violet end is completely absorbed to 52, darkened to 52·5. On opening the slit a little, the dimmed blue comes through from 48—42. The band in the blue now sharply ends at 52, gradually at 48. Diminishing the thickness of the blood to 8 mm. produces nearly the same spectrum, the band in the red being a little narrower, while the band at D cannot be detected. On diminishing the thickness still further to 1 mm., another band appears in the violet. The spectrum is as follows:—The characteristic band from 65—70, the chief band

of the blue end 48—51, the second band of the blue end 45—46·75, the violet being absorbed at 41. The second band of the blue end is much fainter than the first band, and it is not seen in a thickness of 5 mm.

(*c.*) *The Pupa of* S. Tiliæ.—The blood was very bright green and perfectly clear. A thickness of 5 mm. was examined with the light of a paraffin lamp. The spectrum showed a very broad band in the red from 64·5—70, while the red and orange were much dimmed. The violet end was absorbed to 50, dimmed to 51, and no blue came through on widening. The green was the only part of the spectrum which was not dimmed. A thickness of about 1 mm. gave the band in the red from 66—69 (but the limits were very difficult to ascertain exactly), and the violet end was absorbed to 49, darkened to 51, while the blue came through on widening.

(*d.*) *The Pupa of* S. Populi.—The blood was examined by the light of a paraffin lamp, in a thickness of 5 mm. The colour was bright yellowish-green. The spectrum showed the broad band from 66—69, while the violet end was cut off at 51, darkened to 52. No blue came through on opening the slit (with this source of light).

(*e.*) *The Pupa of* S. Ocellatus.—The blood is of a bright yellowish-green colour (rather pale). I have examined many specimens by sunlight. A thickness of 4 mm. shows traces of the band in the red, and with great care and the right conditions of light, gives three bands in the blue end as well as an absorption of the extreme end. The chief band is from 48—50·3, the second from 46·75—45, and the third, which is very faint (and best seen when the slit is widened until the violet is absorbed at 41), occupies 43—42. With less thickness the characteristic band in the red disappears, while with greater thickness it becomes very distinct. After adding absolute alcohol to the blood, a bright yellow solution of xanthophyll was obtained, which gave the characteristic spectrum (shifted to the violet) 49—47, 45·25—44, the violet being absorbed at 42.

(*f.*) *The Pupa of* S. Ligustri.—I have also examined the blood of many individuals of this species by sunlight. The colour is a slightly greenish-yellow in individuals that have fed upón lilac in the larval state, yellow in those that have fed upon privet. The spectrum is marked by very slight effects at the red end, but very powerful absorption of the violet. In a thickness of 35 mm. the absorption in the red was strong, but did not approach that of a thickness of 23·5 mm. of the *P. Bucephalus*, although the absorption of violet was much more complete. The spectrum of this thickness of the blood of *S. Ligustri* is shown in the Chart, Spectrum 2, whilst Spectrum 3 is the *Bucephalus* blood which contrasts with it very markedly at both ends of the spectrum. The characteristic band is seen to extend from 70 (ending sharply) to 64·5, becoming gradually continuous with a less

absorption extending to D, and of which the part from 59—60 corresponds to the second band of the less refrangible part of the spectrum and the third band of true chlorophyll. The violet end is completely absorbed from 51·5, dimmed to 52, but on widening the slit a little blue comes through on the violet side of 48; but very dimly.

A thickness of 3 mm. gives no absorption of the red end, but shows three bands at the violet end and an absorption of the extreme end. The bands are (1), the chief band, from 50—48; (2), the second band, from 46·25—45; and (3), the third band, from 42—43; the violet being absorbed at 41. The bands become less distinct in the order above given, and between them the spectrum is dimmed. The second and third are best seen by an illumination from the bright sky near the sun rather than from the sun itself. This spectrum is shown in the Chart, Spectrum 6.

Comparing the spectra of the blood from pupæ of which the larvæ had fed upon different foods, it was found that the lilac-fed individuals showed greater effect at the red end than the privet-fed individuals, while the converse was true of the violet end. The comparison was made in a thickness of about 8 mm. and by sunlight.

(*g.*) *The Pupa of* C. Elpenor.—A single specimen of the blood of this pupa (from a brown larva) was examined (February 19th, 1885). The colour was a clear reddish-brown in thick layers, almost colourless in a thickness of 3·75 mm. It was examined in thicknesses of 23 mm. and 8 mm. In the former the spectra of derived chlorophyll and xanthophyll were distinct (although the colour was not perceptible). There was a faint band in the red from 69·5 (ending sharply), gradually lessening to 65. There was the chief band at the violet end from 50·5—48, and the second band was present from 46·25—45, and even the third band from 43—42, the violet end being absorbed at 41. These appearances were only seen in certain conditions of light. The first to disappear is the third band, for it fuses with the absorption at the end of the violet, which, therefore, seems to be absorbed to 43. This is common in other cases also. The lesser thickness also showed these effects at the blue end, but not at the red end.

(*h.*) *The Pupa of* D. Vinula.—The blood (of a single specimen) was of a reddish-yellow colour (by lamplight). It was examined in a thickness of 2·75 mm. by the light of a paraffin lamp (December 29th, 1885). There were no bands visible, but the violet end was absorbed up to about 50.

(*i.*) *The Pupa of* P. Machaon.—The pupæ are yellow, or yellow and green, or grey with dark markings. The blood of all varieties is bright yellow. The fresh blood in a thickness of 3·5 mm. gave the following spectrum in very good daylight. The chief band from 50—

48, very distinct; the second 46·25—45, less marked but distinct; the third 43—42, very indistinct; the violet end absorbed at 41. In all but the best light the violet end was absorbed to 43, and the third band ceased to exist separately. A thickness of 8 mm. tends further towards this absorption to 43, and the chief band extends to 50·5, or even a little further. The spectrum was the same in blood from all varieties. An alcoholic extract (90 per cent. spirit) of a grey variety was bright orange-yellow, and gave nearly the same spectrum as the blood, except that the bands were shifted to the violet end, and the third faint band was absent. The spectrum was: chief band 49—47, second 45·5—44, violet end absorbed to 42. The extract was examined in a thickness of 23 mm. and 9·5 mm.

A drop of water was placed upon a cover-glass and the latter was inverted upon the yellow back of a living pupa of *P. Machaon*, which was then examined by reflected light, being entirely opaque. The following spectrum was indistinctly seen:—A band in the red (very faint) 65—63, the chief band 49—47, the second 45·5—44, the violet end being absorbed to 42. The green parts of the same pupa, similarly treated, gave the following results:—69—64 in the red, 52—48. Pieces taken from the yellow parts of the pupal covering gave a similar spectrum to that above described when strong sunlight, concentrated by an Abbé's condenser, was passed through them; the green parts were more opaque, but the spectrum gave the chief bands distinctly. However treated, the spectra of the derived pigments still contained in the pupal covering are very indistinct. This is probably due to the nearly solid condition in which the pigments exist in association with some animal basis. The alcoholic extract of the covering, with its internal surface carefully washed to remove adherent blood, &c., gradually becomes yellow with dissolved xanthophyll. The green parts, carefully washed, dried, and placed in carbon bisulphide, gave a yellow solution with a slight greenish tinge, which gave this spectrum in a thickness of 24 mm. Band in the red 70—66, chief band in the blue 51·5—48, the violet being absorbed at 44. These appearances were indistinctly made out in bad light.

(*j*.) *The Pupa of* Ephyra Punctaria.—The pupa is green anteriorly, especially on the wings, and examined as an opaque object by means of Abbé's condenser (and a stop which blocked the central rays), with good sunlight, the following spectrum was seen:—Band in the red 70—65, a faint but distinct band 63—61, and the band 48—52, or 51 when the slit was widened; the violet absorbed at 43. This spectrum is remarkable as being the only instance yet observed in which the second band from the red end, of true chlorophyll, was seen separately, and occurred in the right order. For in all other pupæ and larvæ the third band appeared after the first, and *then* the second as a shading connecting the former two. The colour is entirely due to the

blood, which, however, does not after removal give a spectrum equal to that of the living pupa, because the amount (from one specimen) is so small, and is disposed in the pupa so as to produce a maximum effect.

(*k.*) *The Larva of* S. Populi, which had been blown and dried many years ago, was of a bright yellow colour (with a few small streaks and patches of green). The larval parieties were washed in water and placed in 90 per cent. spirit, which became bright yellow in a few hours, and gave the two bands of xanthophyll (shifted to the violet).*

(*l.*) *The Ova of* E. Angularia were of a yellowish-green colour. A single ovum examined as an opaque object gave the following spectrum: 51—48, 46·75—45, the violet being absorbed at 43. Although I could not detect the band in the red, I believe that a small proportion of chlorophyll exists in the ovum, as there are some bright green patches surrounded by the far more abundant yellow. The same spectrum was seen in the contents of the ovum which were also better treated by reflected light. The bands of xanthophyll were seen in the alcoholic extract of the crushed ova, the violet being absorbed to 43.

(*m.*) *The Ova of* S. Tiliæ, Ocellatus, *and* Sphinx Ligustri were taken from the bodies of moths which had been in my store-boxes for many years (8—10), and were well washed to remove any of the abdominal contents which adhered to their outer surfaces. The ova were then crushed in a mortar and extracted by 90 per cent. spirit, which in all cases yielded a yellow solution giving the spectrum of xanthophyll.

10. *Comparison of the above Results with those yielded by unaltered Plant Pigments.*—It is of importance for me to give the result of the spectroscopic examination of leaves by the use of the same instrument as that employed for the pigments in the larvæ and pupæ.

Two calceolaria leaves were superposed, gently compressed, and examined by sunlight, giving the spectrum shown on the Chart (Spectrum 4). There is a distinct black band from 70—64·5, two faint bands from 63 (nearly) to a little past 61, and from 60 (nearly) —57·5. There was also a dark band from 51—47·5, with the dimmed blue coming through between this and 43, from which the violet end was completely absorbed.

When five leaves were treated in the same way, results were obtained which are shown on the Chart, Spectrum 5, the three bands at the red end being wider, and the second and third much darker, while the violet end was completely absorbed up to 51, darkened to 52. The same results were seen in the leaves of other plants.

* After twelve hours the spectrum was as follows, in a thickness of 35 mm.:—chief band, 47—49, very distinct; second band, 44—45·5, faint but distinct; the violet end absorbed at 43.—May 4th, 1885.

Comparing these two spectra with those of green blood (not yellow) (Chart, Spectra 1, 2, and 3), the resemblance is seen to be very great, the chief differences being in the second and third bands of the red end, which are continuous (Chart, Spectra 2 and 3), while the third is developed before the second (Chart, Spectrum 1). Considering the chemical change which must have taken place in the chlorophyll during digestion, rendering possible the passage of the walls of the digestive tract, and considering its chemical union with a proteïd constituent of the blood, the resemblances of the spectra are very striking; in fact, the two spectra are far nearer to each other than the ordinary spectrum of chlorophyll in alcoholic solution is to the unaltered chlorophyll of leaves. I believe, however, that the perfectly fresh alcoholic solution gives a spectrum resembling that of the leaf, but the spectrum changes in a few seconds, so rapidly is the solution acted on by light. I have never seen a band in the green in caterpillar's blood, and yet the blood is acid (Mr. Sorby states that any distinct band in the green is due to acidity).* I have not been able to satisfy myself of the fluorescence of green caterpillar blood, certainly there is not the marked red fluorescence of the alcoholic solution of chlorophyll. But this red fluorescence is not present in leaves, nor in the perfectly fresh solution, according to J. Reinke ("Bied. Centr.," 1884, 692—696, abstracted in "Jour. of Chem. Soc." February, 1885, ii, 182). The points of difference between the derived xanthophyll spectrum of caterpillars (shown in Chart 6) and that figured by Mr. Sorby ("Proc. Roy. Soc.," vol. 21, No. 146, p. 442) is the occurrence of a third band in the former, the variable thickness of the second band and difference in darkness between it and the first, and the complete absorption of the violet end. It is possible that the latter may be due to one or more of the lichnoxanthines, while the third band is not always present, and is probably that to which Mr. Sorby alludes when he states (concerning the xanthophylls) that "sometimes three absorption-bands may be seen; but in that case it is generally easy to prove, either by chemical or photochemical methods, that the third band is due to a second substance." This third band was only seen in the best daylight with the greatest care, and it was never present in the alcoholic solution obtained from larvæ and pupæ, which gave the other two bands. It is rather a barren question to discuss whether more than one derived xanthophyll is present, until the botanists have agreed upon the classification of these substances

* The spectrum of Ephyra blood is the same as that of the leaf, except in the absence of the third band; the slight difference in the chief band in the red being explained by difference in the amount of pigment examined. I think it is very probable that a sufficient thickness of this blood will give a spectrum identical with that of the leaf, but nevertheless the derived pigment must have been changed in such a way as to render it stable in solution.—May 4th, 1885.

(or even whether there are more than one. The immense complexity of the subject and the many shades of opinion are seen in a paper by Dr. A. Tschirch, "On the Preparation of Pure Chlorophyll," "Journ. of Chem. Soc.," February, 1884, p. 57).

Nevertheless, the difference between the spectrum of the yellow covering of *P. Machaon* and of its yellow blood, is just that figured by Mr. Sorby in his paper already quoted—a shifting of both the bands towards the violet end in the former case. At the same time it is probable that there is a difference in the animal substance with which the pigment is associated in these two cases, and this may account for some difference. The chlorophyll cannot have exercised a modifying effect on the results given above as due to xanthophyll (Chart, Spectrum 6), for when the spectrum of the latter was seen in the unaltered blood, the thickness employed was so slight that the band of chlorophyll in the red could not be seen, and further yellowish blood was made use of. In the blood of *P. Machaon* the bands of chlorophyll could not be seen even in a considerable thickness. When green blood (*e.g.*, *P. Bucephalus*) was examined for xanthophyll, the latter was obtained in ethereal or alcoholic solution (the chlorophyll always disappearing with the latter solvent, becoming fixed in the solid state with the latter). On the other hand it is certain that the bands of xanthophyll are present in the spectrum of green blood (such as *P. Bucephalus*, Spectrum 3), the first band being contained in that from 52—48, and the second being rendered invisible because of the greater darkening of the violet between 48 and 42. The extreme absorption of the violet end shown in Spectrum 2 (*S. Ligustri*) is due to chlorophyll, xanthophyll, and, possibly, lichnoxanthine; and in this general absorption the chlorophyll takes a small part as compared with the next spectrum (Chart 3), as is at once seen on comparing their absorption at the red end.

11. *Conditions under which the Derived Pigments exist in the Larvæ, &c.*

There is one essential difference between the chlorophyll (especially) in solution in caterpillar blood or tissues, and all other solutions of the same substance, in the great stability of the former under the prolonged action of light. The necessity for this is obvious, inasmuch as the colour of the larvæ often depends upon their complete translucence (*P. Meticulosa*, &c.), and very many of them rest in such a position as to be freely exposed to direct sunlight. Furthermore there are long pauses in larval life, during which no food is taken to renew the derived pigments. Such occur at the various ecdyses, and the colours do not fade at these times, nor when the larvæ are from any cause deprived of food. Hence it is seen that the pigments, having

reached the blood or tissues, are in a state of great stability, and do not require renewal. The same thing is even more forcibly shown in the persistence of the pigments in the pupæ, and on into the ova and young larvæ of the next generation. But comparing these young larvæ before and after their first meal, and remembering that their latter appearance more resembles the one that they will permanently keep (if they remain green), it is obvious that something has been lost to the derived pigments in the interval between pupation and the hatching of the ova (often six months, or longer); and the constituent that has been partially lost is the more unstable of the two under all circumstances—the chlorophyll. It is not likely that the chlorophyll is partially eliminated by the action of light, for the pupal period is in nearly all cases passed in the dark. But it seems probable that pigment is lost in the various phases of redistribution gone through, as solution in fluid and deposition in tissue alternate. It is perfectly certain that the essential difference in colour between the newly-hatched larva (of a green species) and one that has fed, is the predominant yellow tinge and translucence of the former, and the green tinge and comparative opacity of the latter (not entirely due to the digestive tract being full).

The same unusual stability is also true of the xanthophyll, and must be due to the same cause, association with a proteid of the blood or tissue. The separation of the pigments from any constituent of the blood is at once effected by the addition of alcohol. If absolute alcohol be employed, the proteids are precipitated with the colouring matter, but the xanthophyll at once enters into solution in the alcohol, colouring it yellow, while the chlorophyll disappears, and the proteids are decolourised. This must be due to the extreme instability of the chlorophyll, at any rate in the presence of alcohol when separated from the proteid, so that it breaks up into colourless compounds when separated from it. The xanthophyll dissolved in the alcohol slowly decomposes in daylight, but lasts for many weeks, the colour gradually fading. Ether, on the other hand, precipitates the combined pigments and proteid in the form of a green jelly (the upper part is generally darkened), and for some hours dissolves no coloured constituent. Eventually, however, it becomes bright yellow with xanthophyll; while if the green coagulum be broken up in the ether more xanthophyll is dissolved, and a small quantity of green fluid containing chlorophyll remains at the bottom. This, however, is not dissolved in ether. The fragments of the coagulum undergo no apparent change.

If on the other hand the blood be protected from the air, the pigments seem to exist indefinitely, and the fluid does not decompose.* I

* In those instances in which the air has been entirely excluded (when a tube

have not yet tried exposing them to the prolonged action of bright sunlight, but they have frequently withstood the effect of its concentrated rays for several minutes during spectroscopic examination, and have been exposed for weeks to ordinary daylight.

But while the pigments exist unchanged in the blood of many larvæ for so long a time, in other species they are entirely destroyed during the comparatively short period preceding ecdysis, when some green larvæ become brown; and conversely the pigments may appear in the blood equally suddenly. The former change must be due to an active destruction or excretion of the pigments, and is probably also accompanied by changes in the digestive tract, whereby no more pigment is passed through its walls. And so also, as will be seen, the proportions of xanthophyll and chlorophyll may be changed during the life of a caterpillar.

The pigments present in the pupal covering of *P. Machaon* yield to solvent agents far more slowly than in any other place in which I have found them, but this may be due to the resistant nature of the tissue in which they are situated. It cannot be said that they are more stable than the pigments in the blood of the green Ephyra pupæ, for in both alike the pupæ are freely exposed to sunlight from autumn to early summer, without the possibility of the renewal of pigment.

It seems quite certain that the derived pigments of the blood and tissues are only protective, and play no further part in the physiology of these organisms. Thus there are no marked differences between the physiological processes of the brown and green individuals of the same brood in a dimorphic species, or in the processes of a green larva which has become brown, or *vice versâ*. It seems that the pigments are entirely harmless, and are often retained when they would have no effect upon colour. Thus in *P. Bucephalus*, the blood is bright green, although the larva and pupa are entirely opaque, while the eggs, I believe, are white. It is possible that in this case the conspicuous colours—which warn enemies that the species is distasteful—have

section has been completely filled and protected by a cover-glass) the blood has generally remained clear and undecomposed. When, on the other hand, the blood has been sealed up, a little air is unavoidably included, and the fluid generally decomposes, although the pigments are not always destroyed. The blood of the pupa of *S. Ligustri* was sealed up in a tube (with a little air) on March 20, 1885. On May 17 the colour and transparency were unchanged, but a whitish cloud had appeared on one side of the tube. On breaking off the end of the tube the contents were found to be putrid and strongly acid, and a large quantity of gas issued under considerable pressure. The whitish cloud was seen to be caused by the presence of large bacteria. The fluid did not clot or blacken on being exposed to the air, and the spectrum of the clear portion showed that the pigments existed unchanged. The spectrum was observed on May 22, five days after the contents of the tube had been exposed to the air and light, and two months after the blood had been taken from the living pupa.—May 22, 1885.

been recently acquired, and in consequence of the complete opacity, there would be no advantage in losing the colour of the blood.

It therefore appears that we must add a fourth class to the three spoken of by Dr. MacMunn ("Proc. Roy. Soc.," vol. 35, p. 387):—"In addition to the classes of chlorophyll-containing animals mentioned by Geddes, namely those which vegetate by their own intrinsic chlorophyll and those which vegetate 'by proxy,' so to speak, or by means of parasitic algæ, a third class must now be added: those which contain enterochlorophyll in their livers, or other appendages of the enteron." This fourth class comprises those animals which make use of a modified chlorophyll (and other plant pigments) derived from their food, because of the protective colour which they acquire from its presence in their blood or tissues.

12. *Notes upon the Physiology and Chemistry of the Blood of Larvæ and Pupæ.*

Some interesting points came out incidentally during the course of this investigation, and they are therefore mentioned, but only as a preliminary treatment of the subject which I hope to examine more completely during the present year. Much of this section is only suggestion.

Reaction.—The blood of all larvæ and pupæ examined (of all colours) was acid to litmus-paper, turning it distinctly reddish, with the exception of *E. Punctaria*, which seemed to be neutral (although only one specimen was examined). The acid which appears to be volatile can be extracted with ether, but I have not been able to obtain sufficient quantities of blood (during the winter) to determine what acid is present. The precautions previously detailed show how impossible it must have been to obtain any accidental mixture with digestive secretions. Besides, I have tested for the reaction of the blood upon a great many occasions, and again and again upon the same species. The corpuscles are well known to be amœboid, and Professor Schäfer tells me that as far as he is aware this is the only instance of their occurrence in an acid fluid.

Coagulation.—The blood clots after a very variable period of time, but generally darkens in about five minutes, ultimately forming a solid black clot which is due to oxidation. If blood be sealed in a tube, the small quantity of oxygen present will form a thin black film on the surface of the blood, and the action then ceases. I have already shown how blood can be kept indefinitely without clotting in a section of tube with a cover-glass over one end, and the other cemented to a glass slide. I have kept the blood of *P. Bucephalus* in this way for a month, quite unchanged, and on then breaking off part of the cover-glass a thick black crust was formed

on the surface, while the blood beneath became translucent instead of clear and transparent. On removing the crust a second thin one was formed, but on removing this no further coagulation took place. If in sealing up blood, or placing it in a tube section, a bubble of air is accidentally included, coagulation takes place round the bubble, but not elsewhere. This black substance is the normal clot, for the injured places on larvæ which have healed are always black, notably the horns of Sphinx larvæ which have been nibbled off by others of the same species. The coagulation takes place after the addition of water, or of a saturated solution of neutral salt (sodium sulphate). The occurrence of a reducing agent in the blood appears to be very remarkable, but it is possible that the substance is capable of again yielding up its oxygen, and so acting as a carrier. I have observed that if fresh blood be added to that which is turning black on the surface, the black clouds are redissolved. If this be not so, it is difficult to see how the blood can be the internal medium for the supply of oxygen in these animals, and one is tempted to the supposition that in the tracheal system we have a means for the supply of oxygen direct to the tissues. I hope to investigate the question this summer. Another suggestion which occurred to me was that the coagulation is a very similar process to the darkening of cuticular pigment on larvæ and the darkening of the pupal covering. It has always been assumed that this darkening is due to light, but it takes place rapidly and completely in pupæ buried several inches under ground, in compact and opaque cocoons, or sometimes in the heart of a tree. Furthermore, I have never observed that any attainable darkness made the least difference to the darkening of pupæ. I therefore think it very probable that this will also prove to be due to oxidation, and possibly to the formation of a substance similar to the black clot of the blood.

The brown and colourless blood darkens as well as the green, but the latter seemed to yield the most marked results, although I have not observed a sufficient number of instances, except in the case of the green blood, to be able to draw conclusions. There is great variability in the time that elapses before coagulation, and I have seen samples that have not clotted at all. From my memory of the blood of *S. Ocellatus* in the larval state, I believe that the blood coagulates far more rapidly than that of the pupa.

Action of reagents.—The effects of *ether* and *absolute alcohol* have been already described. The blood experimented upon was that of *P. Bucephalus.*

Fifty per cent. spirit, if poured carefully, lies on the top of the blood, and there are white clouds of precipitated proteids just above the junction. If shaken, the proteids and pigments are precipitated as yellowish-green clouds, and in a few minutes the upper part of

the liquid becomes blue, and ultimately black, from the formation of coagulum. The proteids are decolourised and sink, the alcohol remaining yellow with xanthophyll (the chlorophyll disappearing). *Absolute alcohol* does not lie on the top of the blood, but mixes with it at once.

Chloroform behaves in the same manner as ether, but it dissolves nothing coloured from the green coagulum; the latter contracts in a few hours, and a clear blue liquid appears between it and the sides of the tube. The exposed surface of the coagulum (the chloroform having sunk to the bottom) rapidly becomes black.

Distilled water, like weak spirit, lies on the top of the blood with a cloud of precipitated proteid (probably globulin) above the junction. On shaking, the cloud disappears, and the blood only seems diluted; if now more water be added (altogether many times the volume of the blood), in a few minutes the whole fluid becomes cloudy, remaining dark greenish. On filtering, a blue solution comes through, which slightly darkens for some hours. With less water the blood coagulates normally, although after a longer interval of time.

Carbon disulphide had no effect for a considerable time. Eventually the blood was coagulated (green) but nothing coloured was dissolved out.

Heat.—The blood of the pupa of *S. Ligustri* was heated in a glass tube in a water-bath; no change was seen till the temperature reached 132° F., when part of the blood became slightly dim. By 141° the whole of the blood was distinctly cloudy, but it was not till 180° that the blood became quite coagulated—solid-looking and opaque, the proteids being yellow with xanthophyll. In the interstices of the clot was a clear yellow fluid. The xanthophyll in the coagulum was easily extracted by ether or alcohol.

13. *The Relations between the Colour of Phytophagous Larvæ and that of their Food-plants.*

Entomologists have been long aware of the fact that the colours of many larvæ vary (within the limits of the same species) according to the colour of the plant upon which they are found. This is especially true of larvæ feeding upon brightly-coloured parts of the plant, such as the anthers or petals. At the same time there has been hardly any investigation of these interesting facts. The numerous instances in which such variations have been observed are, I believe, exhaustively recorded by Mr. Raphael Meldola in the editorial notes to his translation of Dr. Weismann's "Studies in the Theory of Descent" (the essay on the Origin of the Markings of Caterpillars), and in a paper in the "Proc. Zool. Soc." for 1873.

The first step in this investigation is to make quite certain of the facts by feeding a sufficient number of larvæ, from the egg, upon the

different foods which have been found to be associated with particular colour variations of the larvæ in field observation. Mr. Meldola mentions a paper by Mr. R. M'Lachlan ("Trans. Ent. Soc.," 1865, p. 453) entitled "Observations on some remarkable Varieties of *Sterrha Sacraria*, Linn., with general notes on Variation in Lepidoptera." In this paper Mr. M'Lachlan gives some valuable data as to *Eupithecia Absynthiata.* The author collected about 100 larvæ of this species. When found upon *Senecio Jacobæa* they were yellowish, upon *Centaurea nigra* reddish, and upon *Matricaria* whitish. When nearly full grown they were all given *Senecio Jacobæa*, but this change of food did not affect the colour of the reddish and whitish varieties. From this Mr. M'Lachlan argued (1) that it was necessary for the larvæ to have fed on the one kind of flower from the egg to acquire the resemblance, and (2) that the colour is not caused by the food showing through the somewhat transparent integument. He thinks that variation may be indirectly caused by the food, and he points out the similarity of internal feeders. He also concludes that the indirect effect of the food in producing protective resemblance is normal, and the facts of opposite colouration must be due to other circumstances.

14. *Experiments upon the Larvæ of* S. Ligustri.

Mr. Meldola quotes (in the editorial notes) Mr. Argent for the observation that this larva is of a darker green when found upon laurustinus than when feeding upon lilac. He also quotes Mr. Davis for the opinion that the larvæ upon ash are of a more greyish-green than those upon lilac or privet. My own experience has always been that the larvæ are much duller in both the ground colour and the purple stripes when feeding upon lilac than when occurring upon privet. Those upon ash resemble the lilac forms. Mr. Davis' expression "greyish-green" exactly explains the difference between the lilac and privet forms (being applicable to the former). The privet varieties have a much brighter yellow ground colour, and the stripes are of a more vivid and redder purple, while the purple in the lilac forms is bluer and altogether duller (often tending towards brownish). Having often observed these differences in the field, I experimented upon some larvæ during the summer of 1884. The larvæ from a batch of eggs were divided into three lots and fed throughout their life upon lilac, privet, and ash respectively. The larvæ upon ash did not flourish, but about twelve of the privet larvæ and six of the lilac arrived at maturity, and without exception showed the differences I have indicated above. The results of these experiments have been communicated to the Entomological Society of London, and the two varieties figured, in a paper not yet published.

15. *Experiments upon the Larvæ of* S. Ocellatus.

The variability of this species must have been observed by many entomologists. In Mr. Meldola's editorial notes there are the following references to this larva:—In 1879 Mr. E. Boscher found ten to twelve larvæ upon *Salix Viminalis* near Twickenham. These larvæ were of a bright yellowish-green ground colour, and all possessed the rows of red-brown spots which sometimes occur on this species. He also found eighteen to twenty larvæ of the same species feeding upon *S. Triandra,* in a locality not far distant, all of which were dull whitish-green above, passing into bluish-green below. None of these latter were spotted. In 1880 Mr. E. Boscher found seven larvæ on *S. Viminalis* in the same locality, similar to those found upon this plant the year before. He also found six larvæ upon another species of *Salix* in the same osier bed, two being of the bluish-green and four of the bright green variety.

It must be well known to all collectors that the bluish-green form occurs on ordinary apple trees in gardens. In 1880 Mr. Boscher conducted some breeding experiments upon this larva at Mr. Meldola's suggestion. The larvæ from three batches of eggs were fed upon *S. Triandra*, *S. Viminalis*, and apple respectively. Only three of the third batch survived, and were all of the bluish-green form.

I have added the following evidence to the case of *S. Ocellatus* ("Trans. Ent. Soc. of London," Part I, April, 1884). *Salix Rubra* and *S. Cinerea* produce the bright green variety, and crab also (as far as I could remember). On the other hand my experience had been that *S. Viminalis* produces whitish-green varieties. Figures of the two varieties are given in my paper. It must not be supposed that the yellowish-green varieties are always spotted, or even commonly so, but I believe that an instance of a spotted bluish-green variety has not been hitherto recorded.

My own experiments upon this species were conducted during the summer of 1884. The eggs were obtained from Mr. W. Davis, of Dartford. The larvæ after hatching were divided into five lots of six each, and they were fed upon the following food-plants:—apple, crab, *Salix Viminalis, S. Cinerea, S. Rubra.* As I was travelling at the time, it was sometimes difficult to obtain the right plants. On such occasions I made use of a plant which I believed (from an examination of its leaves) would produce the same colour as the one I had started with. The only instances of this were—once or twice in the case of crab, when I substituted an apple with similar leaves (green and glabrous underneath), and in the case of *S. Rubra*, for which *S. Babylonica* was given once or twice and *S. Triandra* once. I had previously ascertained (in the summer of 1883) that no effect is produced by feeding a larva for several days upon a food-plant which

tends to produce an opposite colouration. Hence the results of the experiments are as convincing as if I had been able to use the same plant throughout (as I did in three cases out of the five).

The larvæ hatched July 15th—18th, and I thought that there was some difference in colour due to the food, as early as July 27th (in larvæ hatched July 18th and fed on *S. Viminalis*), when the larvæ were about 15 mm. long, and advanced in the second stage. On careful comparison, the backs of those feeding on *S. Viminalis*, apple, and crab appeared very slightly whiter green than those feeding on *S. Cinerea* and *Rubra*. Before this all the larvæ were yellowish-green, much resembling the brighter variety. These early differences were *very* slight, and remained so for a long time. The head and two or three anterior segments were yellow-green in all the larvæ up to the end of the third stage, but this colour extended backwards for a less distance and was less marked in the whiter varieties. While there seemed to be some differences, all the larvæ were of a much whiter green than at first. In the third stage there was a blue tinge (especially seen in those feeding on apple) about the whitish-green on the backs of the whiter varieties, which became distinct on placing them beside the others. The white bluish-green appears on the back just above the subdorsal, and this line forms the boundary between the lighter area above and the darker surface below. Sometimes a line along this margin, even lighter than the colour of the back, indicates the position of the old subdorsal.

There was not this sudden transition between the upper and lower surfaces in the *S. Rubra* and *S. Cinerea* larvæ, although the subdorsal was sometimes recognisable. The larvæ feeding on crab were of the lighter variety, contrary to my expectations. As the larvæ advanced in the third stage, the differences increased and became quite distinct, but they did not approach those observed upon adult larvæ in the field. In the fourth stage the differences still continued to increase, although but slightly. The heads of the whiter varieties were still yellowish. It was very noticeable now, as at all times, that the apple, crab, and *S. Viminalis* larvæ were much whiter and nearer the well-known variety than the *S. Rubra* and *Cinerea* larvæ were yellow-green, like the other variety. Furthermore, the *S. Rubra* larvæ were rather yellower than those feeding on *S. Cinerea*. Towards the end of the fourth stage the whiter larvæ (except those on *S. Viminalis*) had become as white as those adult ones commonly found upon apple, and exactly resembled them in colour. On the other hand, the *S. Rubra* and especially the *S. Cinerea* larvæ were far from resembling the yellowish-green variety usually found on these plants, although very different from the larvæ upon apple, &c. The same facts were true of the last stage (fifth). The *S. Rubra* larvæ were the yellowest green, and as the effect of the apple, &c., had been so much stronger

than the *S. Rubra*, &c., the food of the yellowest variety was changed, and it was fed upon apple from the beginning of the fifth stage (August 14th. The larvæ were hatched July 17—18).

By August 23rd most of the larvæ were full-fed, and a careful comparison was made, with the following results:—

Apple.—The five adult larvæ are quite typical whitish-green forms.

Crab.—Only one of the five larvæ is nearly adult, the others far behind. The larvæ were hatched a day earlier than any of the others (July 15), and yet they have been backward all through. This is probably an effect of the food. The larvæ are almost as white as the others (becoming quite so afterwards).

S. Viminalis.—The four adult larvæ are not so white as the apple or crab, but are almost intermediate forms.

S. Cinerea.—The four larvæ may be called intermediate, and they much resemble the *S. Viminalis* forms, but are as yet younger and rather smaller (the tendencies towards the two varieties slightly increasing with growth).

S. Rubra.—The four adult larvæ are far more different from the apple varieties than any of the others, but they are not greatly beyond intermediate forms. The one fed upon apple for nine days was the brightest of all; it is now rather whiter than the others, which were still fed on *S. Rubra*, although it does not approach the apple or crab larvæ. But any change at all (as proved by the experiments to be described) in so short a time shows the very strong tendency of the whole of these larvæ towards the white variety. On August 26th this larva was adult without further recognisable change in colour.

This then was the result of the experiments. No one who compared the *S. Rubra* with the apple larvæ could hesitate for a moment in coming to the conclusion that the ground colour is largely affected by the food-plant. But at the same time there was no doubt that this is not the whole explanation of the differences observed in larvæ in the field, for my *S. Rubra* and *S. Cinerea* larvæ were not yellow-green varieties, but only intermediate. A strong tendency was manifested in the larvæ all through towards the white variety, and the food-plant could only overcome this to the extent of producing an intermediate form.

16. *Observation in the Field upon Larvæ of* S. Ocellatus *during* 1884.

I will now describe the results of my observations in the field during the past summer, which throw great light upon this question. The larvæ of *S. Ocellatus* were abundant last year, and I will give a list of all found to indicate the proportion of forms which were of exceptional colour (in relation to their food-plants).

August 4th.—Upon *S. Viminalis* on the River Cherwell, near Oxford, an almost full-grown larva of the whitish-green variety (normal according to observation).

(About) *August 7th.*—Upon (probably) *S. Ferruginea** (Anderson), on the Cherwell, two almost full-grown larvæ of *nearly opposite varieties and both on the same tree.* One was a bright yellowish-green variety (I believe abnormal on this tree), the other inclining towards the whitish-green side of an intermediate form (probably representing the tendency of this food-plant, and therefore normal).

(About) *August 7th.*—On *S. Rubra* (Cherwell) three larvæ; two just entered the last stage, one quite small in the third stage. All yellowish-green (normal), and one of the larger ones with brownish-red spots round all the spiracles except the first.

August 11th.—On *S. Ferruginea* (Cherwell: the same tree as that on which the two opposite forms were found, as described above); two larvæ at the close of the fourth stage, and both inclining to the white side of an intermediate variety (probably normal).

Also on *S. Viminalis*, a full-grown bright yellowish-green larva (abnormal according to my observations). The larva may have wandered from a neighbouring *S. Rubra*, but this is not likely, as the *S. Viminalis* was much eaten and the other apparently not eaten at all. On other occasions I have found the interlacing branches of *S. Viminalis* and *S. Rubra* eaten in such a way as to suggest that a larva had wandered from one to the other.

August 14th.—Upon *S. Smithiana*, at Botley, near Oxford, a full-grown larva inclining to the yellow side of an intermediate variety (probably slightly abnormal). Also upon *S. Smithiana* at Boar's Hill, near Oxford, a full-grown larva inclining to the white side of intermediate variety (probably normal). Also upon *Pyrus Malus* (var. *Acerba*) at Boar's Hill, an adult, very bright yellowish-green larva (probably normal). In this last case there were the brownish-red patches in front of and behind the eight posterior spiracles, but none of the other series of spots which are sometimes present.

August 16th.—Upon *S. Rubra* (Cherwell) an adult intermediate variety (abnormal).

August 17th.—Upon a small tree of *S. Babylonica*, in a garden at Oxford, six larvæ, of which five had just entered the fifth stage, or were passing through the last ecdysis. These were intermediate

* The tree in question (of which I have only seen a single specimen) was referred in the autumn of 1884 by Rev. J. E. Leefe and Mr. G. C. Druce to this species, while Mr. Bennet suggested *S. Rugosa.* This spring (1885) I have submitted the catkins to Mr. G. C. Druce, who has kindly sent specimens to other authorities, with the following results: Mr. S. G. Baker, of Kew, states that it is typical Smithiana, while the Rev. J. E. Leefe and Mr. Druce adhere to their former opinion that the plant is *S. Ferruginea.*—May 22, 1885.

varieties with (at present) more or less tendency towards the white-green variety, but on the whole very uniform (probably abnormal). One larva was rather more advanced and well in the fifth stage. It was a bright yellowish-green variety (probably normal).

August 18th.—Upon the same *S. Babylonica* one more intermediate variety, similar in size and colour to the five found on the 17th (probably abnormal).

September 2nd.—Upon an apple in a garden at Reading, a nearly adult bright yellowish-green variety (rather whitish on the back, but not nearly so much so as an intermediate variety). The apple leaves were white underneath, and in all previous experience such a tree has produced the most extreme form of whitish-green larvæ. Hence the occurrence is highly abnormal. In addition to the above, two larvæ were sent to me—one whitish-green variety, found upon the ground of another garden in Reading, and doubtless feeding on apple, and normal: another from the same garden as that in which I found the larva on September 2nd. This larva had turned brown, and was found upon a gravel path, but it was described as bright yellowish-green, and it bore traces of having had this colour when it reached me. It was found in the part of the garden where the apples grow. In quite another part at a considerable distance there is a tree of *S. Babylonica*, but it is not at all probable that the larva fed upon this plant. On the whole there is strong evidence for believing that this larva was another bright yellowish-green variety feeding abnormally upon apple.

17. *Experiments upon Captured Larvæ of* S. Ocellatus

Experiments were made upon some of these larvæ to ascertain if it was possible, by changing the food, to modify their colours after a definite tendency had appeared. Such experiments could of course only be applied, with any hope of success, to the youngest of the above-mentioned larvæ.

The bright yellowish-green variety in the fifth stage (with brownish-red patches) found August 7th, on *S. Rubra*, was given apple August 8th, its length (when extended) being 40 mm. On August 14th, this larva was 58 mm. long when measured at rest in the sphinx attitude. It was now full-fed and became brownish. The colour was quite unchanged up to this point, and thus an immense amount of food can be eaten and great additional weight gained without any effect on the colour.

The other very similar larva found on *S. Rubra*, August 7th, was not quite so yellow as that just mentioned, but by August 10th it had become about the same. It was now fed upon apple, and was adult (and turned brownish) August 18th, without any recognisable dif-

ference in consequence of the change of food. The small larva (in the third stage) found upon the same plant, on the same date, was also fed at once upon apple. At this early stage the larva was as bright a specimen of the yellowish-green variety as I have ever seen. The apple made no difference in its colour, although it was eaten for many days. The larva had been slightly injured, and died without entering another stage. Evidently it possessed an even stronger tendency towards the yellowish-green variety than my bred specimens showed towards the other form, since the colours were very marked, while the larva was so small.

The two whitish larvæ in the fourth stage, found on August 11th, on *Salix Ferruginea*, were fed upon *S. Triandra* or *S. Rubra* during the whole of the fifth stage. On August 24th these larvæ were adult (and one of them browned): some slight effect had been produced, for they were both of them less white than was to be expected from their appearance when found.

The seven larvæ found August 17th and 18th upon *S. Babylonica*, were fed upon the same food-plant, and carefully compared. The bright yellowish one, which was more advanced than the others, became adult (and turned brownish) August 25th. It remained much the brightest all through. The others became adult a few days later, and throughout remained intermediate forms with a variable tendency towards the yellowish variety.

The tendency of the food-plant was seen during the growth of these six larvæ in the fifth stage, for they were whitish intermediate varieties at the beginning of this stage, and they inclined the other way at its close. The other larvæ were too old for experiment.

18. *The Protective Character of the Variation in the Larvæ of* S. Ocellatus.

These larvæ are protected by resembling the colour, and, to some extent, the veining of the underside of the leaves of their food-plants. The relations between the ground colour of the larvæ and the undersides of the leaves are shown in the following table, as far as these data have been indicated by any observation or experiment hitherto recorded:—

Food-plant.	Effect on *S. Ocellatus*.	Colour of undersides of leaves.
Apple	Strongly tends towards whitish varieties according to all observers, and also proved by two sets of experiments (Mr. E. Boscher's, quoted by Mr. Meldola, and my own). Exceptions *very* rare, and only known in 1884.	White undersides: pubescent.

Food-plant.	Effect on *S. Ocellatus.*	Colour of undersides of leaves.
Crab (cultivated)	Probably variable. I have found yellowish varieties on "yellow Siberian crab." My experiments showed that larvæ with a tendency towards whitish were not modified.	Undersides vary from green to white, but are smoother than apple, and sometimes glabrous.
Crab (wild) .... Var. *Acerba*	Probably strongly towards yellowish, but only one observation (which supported this view).	Undersides distinctly green and glabrous.
S. Viminalis ...	Apparently contradictory results. From a fair number of observations in the field (with only one exception) I have assumed that the effect is towards the whitish variety. Mr. Meldola's instances, which are about 18 in number) oppose this conclusion. My breeding experiments perhaps supported Mr. Meldola's instances rather than my own, but they were not very conclusive. My experiments showed that this plant does not produce so white a green as apple, and this I think is the case. But it certainly does not cause so yellow a green as *S. rubra.* I am still inclined to think that it may tend towards whitish, but the experiments must be repeated.	Undersides white, with dense satiny down.
S. Smithiana ...	Two instances, one inclining towards the yellow side of intermediate, one towards the whitish side. The latter, I think, represents the real tendency.	Undersides white, with dense satiny down.
*S. Ferruginea,** *Anderson* (probably)	Four instances upon a single tree, doubtfully referred to this species by Rev. J. E. Leefe. The leaves were exactly like those of *S. Smithiana,* only smaller. Three of the larvæ inclined towards the whitish side of intermediate, while one was bright yellowish - green. The former is doubtless the real tendency, and if so, indirectly supports my impression as to the tendency of *S. Viminalis.*	Undersides white and rather silky.
S. Cinerea......	Only one instance, the yellowish variety, upon a tree of which the leaves had dark or glaucous undersides, and were not downy (at any rate distinctly). This probably represents the tendency of that particular tree, but in many cases it is	Undersides reddish, glaucous, or ashy, downy sometimes, and varying in the amount of down when present. A *very* variable species.

* See note upon this species in paragraph 16.

Food-plant.	Effect on *S. Ocellatus.*	Colour of undersides of leaves.
	likely that the opposite effect may be produced when the leaves are downy and white underneath. My experiments produced intermediate forms, but they are not very conclusive, for the larvæ could not be fed upon the same variety of *S. Cinerea*, as I was travelling about at the time. On some occasions I was obliged to use leaves with white pubescent undersides, which I believe tend towards whitish-green.	
S. Triandra ...	Mr. Meldola's instances (about 19) point towards the tendency being in the direction of whitish-green. My own experiments upon the modifying effect produced upon the whitish larvæ by feeding on this plant throughout the fifth stage, rather tend towards the other direction. From the colour and surface of the leaves, I should certainly have thought that the tendency is strongly towards yellowish.	Undersides green and glabrous.
S. Babylonica...	Of seven instances, one was yellow and six intermediate, inclining towards the yellowish side. The former I believe represents the tendency of these leaves, which much resemble those of *S. Rubra*.	Undersides green and glabrous.
S. Rubra......	A fair number of instances in favour of a very strong yellowish tendency. Only one exception (intermediate). Experiments confirm the observations, as yellowish intermediate forms were produced from larvæ, strongly tending towards the whitish variety.	Undersides green and glabrous.

Looking at the above list, I am strongly convinced that the tendencies of one of the food-plants of this larva can be ascertained by looking at its leaves, and that leaves with white pubescent undersides, as a rule, produce whitish varieties, while those with green glabrous undersides generally produce yellowish forms.

The greatest support to the theory that the colour and texture of the undersides of leaves is the best criterion of the effects is shown by the very strong cases of apple and *S. Rubra*. The same theory is supported by crab, *S. Cinerea*, and *S. Ferruginea*, as far as their evidence goes, and *S. Smithiana* is certainly not in opposition to it. *S.*

Babylonica is also strong evidence, if we assume (as there is some reason for doing) that the six intermediate larvæ had tended towards the whitish variety. *S. Triandra* and *S. Viminalis* remain a difficulty, but there is some confliction of testimony. The cases of *S. Smithiana* and *Ferruginea* are favourable to my view of the effects of *S. Viminalis* and of *S. Triandra*, although indirectly, for the leaves of the former species are intermediate between those of *S. Caprea* and *S. Viminalis* (*S. Smithiana* may be a hybrid between these two, and *S. Ferruginea* a variety of *S. Smithiana* or a hybrid between it and *S. Cinerea*. The leaves of the *S. Ferruginea* spoken of exactly resembled those of *S. Smithiana*, except that they were smaller.) Hence if *S. Viminalis* tends towards yellowish, it would be expected that *S. Ferruginea* would tend at least as strongly in the same direction, for its leaves resemble those of *S. Cinerea* as well as *S. Viminalis*, and *S. Cinerea* produces yellowish larvæ. And so with *S. Smithiana*, which resembles *S. Viminalis* and *S. Caprea* (which probably produces, yellow larvæ from its resemblance to a large *S. Cinerea*). But both these intermediate sallows produced, as a rule, intermediate varieties—that is to say, their effects were between *S. Viminalis* and *S. Cinerea* (or ? *S. Caprea*), just as their leaves combine the characters of these forms. And, further, in the intermediate varieties produced, the whitish form predominated, just as the characters of *S. Viminalis* predominate on the underside of the leaves. Hence the observations upon these two food-plants support my view as to the effect of *S. Viminalis*. In the case of *S. Triandra* I have some little experimental evidence, but I have never found a larva on this tree in the field. I feel strongly that its true tendency is, like that of *S. Rubra*, towards a protective (*i.e.*, a yellowish) variety. This year I hope to breed a large number of larvæ from the egg upon this tree, and upon *S. Viminalis*. Omitting these two trees, the whole list is very favourable to the theory that the results are protective, and that the tendencies can be ascertained by looking at the undersides of the leaves.*

19. *The Complex Nature of the Influence of the Food-plant upon the Larvæ of* S. Ocellatus.

The simplest view of this matter would be to suppose that the colour of the leaf was itself the cause of the colour in the larva, showing through the transparent skin, and that the change of diet was therefore the direct cause of the change of colour. But it is quite clear that the influence cannot be of this simple kind, for the amount of substance which forms the underside of the leaf is a very small proportion of the whole substance that is eaten.

* I have to thank Mr. G. C. Druce for much kind help in the difficult task of naming the sallows.

It is, therefore, hardly conceivable that this small proportion can *directly* modify the whole larva into its own likeness;—that the under surface eaten by the larva should cause the latter to resemble the surface itself. And yet it is only this part of the food which is common to the various food-plants producing similar effects. In everything except whiteness and pubescence of the underside, the leaves of the apple must differ more widely from those of *S. Viminalis*, *Ferruginea*, and *Smithiana* than these do from *S. Rubra*. And yet the effects of the latter plant are very different from the three former species of sallow, of which the tendencies incline towards those of apple. And further, the resulting larvæ are far better protected by such effects produced in common by apple and certain sallows than if all the sallows had tended in the same direction. Furthermore, the appearance of the undersides is quite as much due to texture as colour, for the pubescence produces a large part of the effect of dead whiteness. But the larva gains the same appearance by colour, for its hairs are microscopic rudiments (only discovered by myself last summer, 1884). Again, there is no reason for supposing that the chlorophyll from the leaves of the food-plants would yield solutions corresponding in colour with the larvæ; in fact, it is certain that this is not the case, for the colour of the upper sides of the leaves do not afford an indication of the effects on the larvæ, and yet the true colour of the leaf is far more distinct here than it is on the under surface.

These conclusions are also confirmed by considering the effects of the experiments. While the existence of an influence of the food-plant upon larval colouration is undeniably proved, the simple theory of its manner of action is entirely refuted, for such a theory does not allow for any powerful tendency on the part of the larva which may even counterbalance that of the food. Thus in the experiments the effects produced were not according to the power of the food-plant (as proved by observation), when the tendency was in the direction of yellowish, while the effects were very powerful when the tendency was in the other direction. And so also with the observations in the field, as was proved by the very different colours on the same tree, and the strongly yellowish variety upon apple. It cannot be argued that the irregularities observed are due to the effects of the food-plant varying in intensity upon larvæ of different constitutions. They can only be explained by the possession of an opposing force in the larva itself, which may not only exactly counterbalance the tendencies of the food, but may produce the result of the strongest opposite tendencies. The former explanation would account for all varieties from intermediate to whitish upon apple, and from intermediate to yellowish upon *S. Rubra*, but it fails to account for anything on the whitish side of intermediate upon *S. Rubra* or on

the yellowish side in the case of apple. Far less can it account for an almost typical yellow variety upon this latter plant.

It is clear from the experiments upon the captured larvæ that some effect may be produced in an intermediate variety by feeding it for some considerable time (such as during the last stage), upon a food-plant known to have strong tendencies. But this treatment does not affect a larva which possesses a well-marked colouration—that is, a larva with strong tendencies itself, and which coincide with those of the food-plant. Nevertheless the experiments with larvæ bred from the egg prove that the variety towards which the larva is strongly tending may be much modified by the food-plant given it during the whole of its life. When such a change has been produced and is manifest, if the food be appropriately shifted at the beginning of the fifth stage, some considerable effect may be produced in the larva, in the direction of the original tendency.

20. *A Suggested Explanation of the Larval Tendency towards Certain Colours Independently of the Food-plant.*

The most probable explanation of these larval tendencies—which differ so much in strength and direction, and which are independent of the food-plant though generally modifiable by it—is that phytophagic effects are hereditary, and thus stronger tendencies accumulate as the larvæ of successive generations feed upon the same food-plant or on those which produce the same effects. In this way all shades of colour may be occasionally found upon any tree, and the same irregularity may be produced by interbreeding between individuals with different tendencies. This explanation accounts for all the facts brought out by this year's experiment and observation, and it is also in harmony with previously recorded facts. Thus the unexpected results of the breeding experiments would be explained by supposing that during the previous generation (or perhaps generations), the larvæ had fed upon apple or some other food-plant tending to produce whitish varieties. The yellowish larva found upon apple must, according to this theory, have descended from a long line fed upon *S. Rubra*, or some plant with leaves having green glabrous undersides. In this case I believe that there must have been several generations accumulating yellowish tendencies by their food-plants, for the effects of apple are so very strong in the other direction, and yet so little change was produced. So also with regard to the two larvæ of different varieties upon the same tree, it may be supposed that there had been different tendencies due to the food-plants of past generations. In one of the cases quoted, there is some slight evidence that this reasoning is correct. Of the seven larvæ found upon *S. Babylonica*, one was yellowish, and the others intermediate.

According to this theory, the egg from which the former was hatched had been laid by a different female from that which produced the others. Such a conclusion is confirmed by the fact that the yellowish larva was decidedly older than the others and became adult some days earlier, the six intermediate larvæ keeping together throughout. I endeavoured to ascertain the past history of the parents of the larvæ with which I experimented, but Mr. Davis informed me that his larvæ had not been kept separate, and had been found on different food-plants.

It may be argued that there is probably great difference among individuals of the same brood in the power of transmitting these tendencies, and that this might explain some of the above-mentioned irregularities. But the breeding experiments, as far as they go, negative such a conclusion. There was very little difference between the larvæ of each separate lot, and considerable differences between the lots (in some cases). The influences of the food-plant seemed to be most rigidly regular in their effects on each division of the larvæ: in fact one might say that there were practically no individual differences in the tendency towards a whitish variety, although the tendency was very strong.

There is one argument against the conclusion that these effects are in any way due to the food-plants (but I do not see how it can stand against the breeding experiments) :—the fact that the variably developed system of brownish-red spots have only been found on the yellowish-green larvæ up to the present time. (Mr. Meldola's instances point in this direction without exception, and so also with my own.) It is, however, quite possible that these spots may be ultimately found upon the other variety. If not, it would seem to imply that the yellowish-green form, often brightly ornamented with red spots, is more ancient than the other, and that reversion to it is caused by certain food-plants. Against this comes the fact that the red markings have all the excessive variability of a reversion character, while the yellowish-green ground colour can be produced with great regularity. Again, the spots occur upon larvæ with all shades of ground colour in *S. Populi.* Looking at all the facts I cannot doubt the cogency of the explanation offered above of the colouration being due to the food-plant, and of the accumulation of the influence during successive generations. It seems very probable that we have here an instance of a larva of one species dividing into two. The tendency is more of this kind than towards true dimorphism, for the two forms have different food-plants. If a long series of generations upon one plant produced a distaste for the other kinds, so that the larvæ wandered or died, it is then certain that natural selection would lead the females to lay their eggs on the appropriate foods, and the separation would be complete. Mere proximity of one kind of food and distance from

other kinds might produce some tendency in this direction. In gardens there are many chances in favour of eggs being laid for several generations on apple, and so with regard to the predominance of any one species of Salix over a particular area.

It would thus be possible for the two varieties to be rendered locally distinct, for the localisation of a food-plant would overcome both causes of variability—the liability to lay eggs on other plants with different tendencies, and the chance of interbreeding between the two varieties. If the two varieties could become stereotyped in this way (and it has been shown that *occasional* recurrence to other kinds of food on the whole produces no effect), there would result a species with specific differences in the larvæ, although the pupæ and imagos would remain identical. Instances very nearly of this kind are, of course, well known. The above would certainly be true of two isolated tracts in which food-plants of opposite tendencies existed to the exclusion of the other kinds. If the tracts were adjacent it would still be true of nearly all the larvæ in each of them.

It may be argued that the food-plants are so intermixed throughout this country that we should not expect to find the uniformity which has hitherto seemed to prevail. But it must be remembered that such uniformity is chiefly observed upon apple, and that the effects of this food-plant are so strong that it needs a very powerful tendency to overcome them. Furthermore, there is a certain amount of local separation between apple trees in gardens and the various species of sallow by the banks of streams and in damp lanes and hedgerows. The species of Smerinthus are inactive and slow flyers in the perfect state; so there is every tendency towards interbreeding between those in gardens, and towards the eggs being laid upon the food-plant which is on the spot. These facts will account for the almost invariable occurrence of the whitish variety upon apple. The case is very different with the sallows. The various kinds occur in close proximity, so that it is very common to find several so-called species of Salix in a single osier-bed. The tendency of sallows as a whole is rather towards the yellowish variety than towards the whitish, but there is at all times the greatest facility for interbreeding, and for laying eggs on many species of food-plant, even in the case of the most sluggish insects. This explains the irregularity of the larvæ when found on sallows, and the fact that the yellow variety is perhaps more commonly found upon the trees with an opposite tendency than the white variety upon trees tending towards the yellowish form. I may add that the garden at Reading in which I found the yellowish form upon apple, is on the outskirts of the town, and so situated that the eggs might easily be laid by a female of which the larva had fed upon sallow, for these trees are common at no great

distance, and in fact there are examples of *S. Cinera* and *S. Babylonica* in the garden itself.

It seems therefore that the explanation offered above must be correct, and if so we have an instance of a character (larval colouration) that is shown in a very obvious manner to be the result of the interaction between the influences at work in an individual life and the inherited tendencies following from the influences that moulded the lives of ancestors. In this case, too, there is a fairly accurate test for the predominance of either element, in observing the resultant tint, and making allowance for the tendencies (already gauged) of the food-plant.

The case of *S. Ligustri* is also to be explained in a similar way, for here, too, we have the difficulty of conflicting experience (Mr. Davis' and my own). I have a considerable number of pupæ of *S. Ocellatus*, with careful notes of the colours and food-plants of their respective larvæ, so that I hope to be able to test the above theory very conclusively in the summer.

21. *The Essential Nature of the Changes in Colour Produced by Food-plants.*

It is first necessary to carefully describe the appearance of the varieties of *S. Ocellatus*, before explaining the underlying cause of the colour, by reference to larval or derived pigment.

The larva of *S. Ocellatus* is covered with minute white points (which are the tubercles at the bases of rudimentary hairs), conferring upon the skin a shagreened texture. The colour is much modified by these dots; and their arrangement, together with the spreading of white areas from their bases, and the coalescence of such areas, form nearly all the markings of the larva. The dots are very variably developed; at one time I thought that the whitish varieties must be caused by their relative predominance, obscuring and modifying the green ground-colour. Careful observation, however, convinced me that the difference is not due to this. The most extreme whitish varieties are almost white upon the back, above the horizon of the old subdorsal line. Below this the bluish-green tint predominates (the line of demarcation being very sharp), while the under surface is dark bluish-green. Careful examination of the white dorsal surface with a lens shows that the green is obscured (and almost obliterated) by a comparatively superficial accumulation of white masses, while the green when present seems to shine through from a lower level. On the under surface there is nothing to obstruct the effect of the green (since the shagreen dots are very minute, and the white masses are absent). The lateral surfaces offer intermediate effects produced by intermediate conditions. An

extreme yellowish variety, on the other hand, does not present the same marked line of demarcation at the subdorsal level; the whole larva is bright yellowish-green, the tint being more pronounced on the under-surface, but very bright and distinct over the whole larva. It is here also obvious that the main colour is deeply placed, and is to some extent modified by the shagreen dots and by subcuticular whitish masses, but these latter have not the same effect as in the other variety (and are probably developed to a much less extent).

A less extreme form of the yellowish variety differs in the dorsal tint being more completely obscured by white (the result being a yellowish-white in this region). The line of demarcation is not sharp. In an intermediate variety (such as that found upon *S. Ferruginea*), the upper surface resembles that of the whitish form, with the same sharp contrast at the subdorsal, but below this the tint is green, without any marked tendency towards yellowish or bluish, the under surface being of course the most deeply coloured. Such a variety is not at all uncommon, and I have described it as inclining towards the whitish side of intermediate. It is truly intermediate in the colour of the lateral and lower surfaces, inclining towards the whitish variety on the upper surface, and in the sharp demarcation at the subdorsal level. It is at once clear that the green tints, whether yellowish or bluish, are due to derived pigments, which have already been described as present in the blood of the pupa, and which have been proved to exist in the subcuticular tissues, as well as in the blood of the larva. The white masses are evidently due to a substance of true larval origin, and the effect of the food-plant upon the latter shows at once that its influence extends beyond the derived colours. The yellowish tint of the bright variety is due to predominant xanthophyll, as is proved by comparing the blood of pupæ of which the history is known. The greener variety must conversely be due to a larger proportion of chlorophyll, and the blue tinge may be caused by a slight change in its constitution; but this needs investigation in the larva, for although the pupal blood of yellow varieties is brighter in colour, that of the others does not retain the blue tinge. The difference is delicate, but distinct; and much must be lost in the redistribution accompanying pupation. It is likely that the blood of the larvæ is not very different, but that the chief distinctions are due to the subcuticular derived pigments, and these are largely destroyed in the changes preceding pupation. It is unfortunate that this inquiry did not suggest itself to me when I possessed the living larvæ in abundance.

The case of *S. Ligustri* is more distinct, for the differences in the green seem to be entirely retained in the pupal blood. I have now many times compared the blood of lilac and privet pupæ, and that

of the former was always greener, and gave the principal band of chlorophyll more distinctly when equal thicknesses were compared. It therefore appears that the blood and subcuticular pigment are of similar tints in this larva. In this case also the influence of the food-plant extends to the true larval pigments, for the purple stripes are much brighter in the privet forms.

There is very little doubt that the green and brown varieties of Noctua larvæ could be produced by a proper arrangement of surroundings, but as the experiment has not yet been tried, it must not at present be assumed that the food-plant could produce so great an effect as the removal of the derived pigments from the blood. Obviously the common experience that brown larvæ rest on the brown parts of the plant or on the earth is not a sufficient argument, as the habit may have followed the change of colour. But a change in the relative proportion of the derived pigments passed through the walls of the digestive tract into the blood seems to result from a more complex influence than that which would lead to the entire removal of pigment from the blood; and yet we have proved that the food-plant exerts the former influence.

22. *Summary; and Conclusion as to the Nature of the Influence of the Food-plant.*

It has been shown that the influence of the food-plant is not uniform, that it must act during a large proportion of the whole larval life in order to produce an effect, that effects of surface colouration due to consistence may be imitated in colour, and it has been rendered extremely probable that the effects accumulate during successive generations. It has been shown that the effects are partially due to pigment which is proper to the larva, and which has no immediate relation to the food-plant, while the changes produced in the derived pigments are even more complicated, and due to the predominance of one or other of the vegetal colouring matters in the tissues and blood, and before this in the materials which traverse the walls of the digestive tract (for the hypothesis that certain pigments are continuously destroyed after passing the digestive tract, until a certain colour is produced, is even more complicated).

Such effects are entirely inexplicable by the simple theory of phytophagic influence with which I was strongly prepossessed on approaching this inquiry. So impossible does it seem that the effect could be produced by the direct influence of the material which is eaten, that it would be wiser to abandon the term "phytophagic," at any rate in the sense of producing these changes. The term still holds good for the broad fact that pigments derived from the food-plant play a most important part in larval colouration, and further than

this, that such pigments afford the material which is moulded by some subtler influence into a likeness to a special part of the environment. Of the nature of this influence we know nothing at present, except that the whole investigation points towards a nervous circle whose efferent effects are seen in the regulation of the passage of pigments through the digestive tract into the blood, and finally the tissues, and in the colour of a certain amount of true larval pigment, while the afferent part of the circuit must originate in some surface capable of responding to delicate shades of difference in the colour of the part of the environment imitated. This interpretation is rendered unusually difficult by three facts: the gradual working of the process, often incomplete in a single life; the excessively complex and diverse result, and the special character of the stimulus (for it is only the part of the environment imitated which produces any effect, *e.g.*, the undersides of the leaves in the case of *S. Ocellatus*, and yet the environment, of course, includes both surfaces). In parallel cases (Amphibia, fish, &c.), as far as any parallelism exists, the stimulus acts upon the eye, and this may be true of larvæ also, but it is useless to speculate on the subject until further data have been acquired by experimental research.

Variations in the colour of the derived pigments in the blood occur apparently spontaneously and uselessly in the opaque *P. Bucephalus*, for there is a great difference in the yellowness (xanthophyll) of the blood of this pupa and in the amount of chlorophyll. Thus it is possible that the variation began in this way, and was afterwards rendered efficacious by co-ordination with the environment. But there are difficulties in the way of any suggestion founded upon observations on this species, for the existence of green blood seems to indicate a different method of colouration in former times, and if so, the variations in colour may be themselves remnants of a past susceptibility to the influence of environment.

EXPLANATION OF SPECTRUM CHART.

Spectrum 1.—The blood of the larva of *P. Meticulosa* (green variety) examined in a thickness of about ·75 mm. by sunlight. The blood had been allowed to remain in an open capillary tube for about four days, and was then sealed up after it had evaporated to half its bulk.

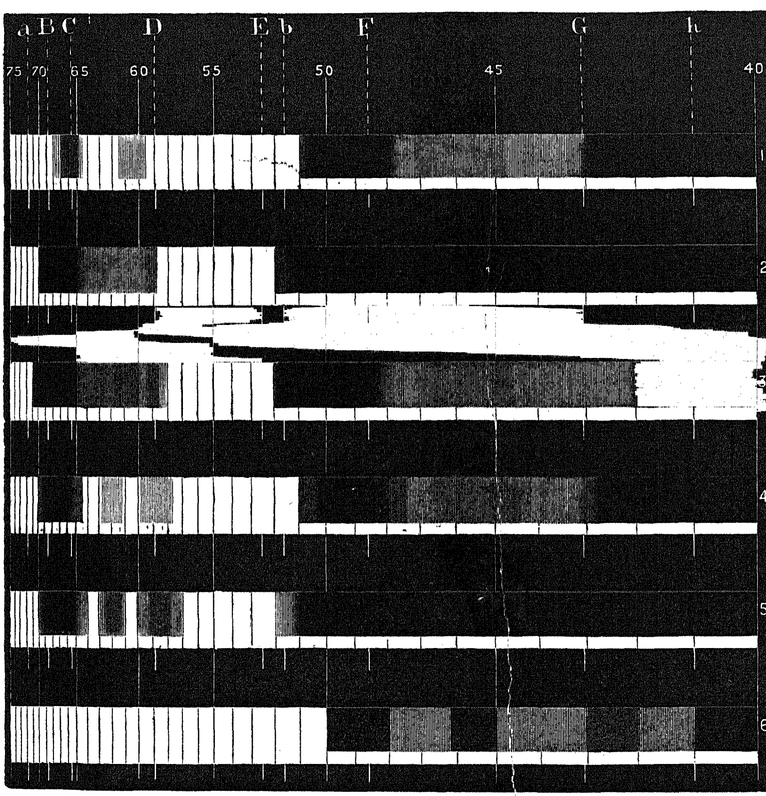
Spectrum 2.—The fresh and unaltered blood of the pupa of *S. Ligustri*, examined in a thickness of 35 mm. by sunlight.

Spectrum 3.—The fresh and unaltered blood of the pupa of *P. Bucephalus*, examined in a thickness of 23 mm. by sunlight.

Spectrum 4.—Two fresh calceolaria leaves, gently compressed and examined by sunlight.

Spectrum 5.—Five ditto ditto.

Spectrum 6.—The fresh and unaltered blood of the pupa of *S. Ligustri*, examined in a thickness of 3 mm. by illumination from the bright sky near the sun.



April 30, 1885.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Abstract of some Results in Elliptic Functions. (Part II.)" By JOHN GRIFFITHS, M.A. Communicated by Professor G. G. STOKES, Sec. R.S. Received April 9, 1885.

1. *On the $z(u)$ Function Complementary to Jacobi's* $Z(u)$.

The double periodicity of the elliptic functions gives rise to an interesting function of the form $z(u)=a-\frac{J'}{K'}u$, where

$$u=\int_0^\theta \frac{d\theta}{\sqrt{1-k^2\sin^2\theta}}, \quad a=\int_0^\theta \sqrt{1-k^2\sin^2\theta}\, d\theta=E(u), \; J'=K'-E'.$$

By changing a, u, respectively, into (1) $a+2iJ'$, $u+2iK'$, (2) $a+2E$, $u+2K$, (3) $a+E-k^2\frac{\operatorname{sn}u\operatorname{cn}u}{\operatorname{dn}u}$, $u+K$, it is easily seen that $z(u)$ satisfies the following relations, viz.:—

$$z(u+2iK')=z(u),$$

$$z(u+2K)-z(u)=\frac{\pi}{K'} \; \left(\text{since } KE'+K'E-KK'=\frac{\pi}{2}\right),$$

$$z(u+K)-z(u)=\frac{\pi}{2K'}-k^2\frac{\operatorname{sn}u\operatorname{cn}u}{\operatorname{dn}u}.$$

2. *Deduction of a $\Phi(u)$ Function from $z(u)$.*

Writing $\Phi(u)=\sqrt{\frac{2k'K'}{\pi}}\,e^{\int_0^u z(u)du}$ we can take the foregoing $z(u)$ relations as equivalent to—

$$\Phi(u+2iK')=-\Phi(u),$$

$$\Phi(u+2K)=\frac{1}{r}e^{\frac{\pi u}{K'}}\Phi(u),$$

$$\operatorname{dn} u = \sqrt{k'}\, r^{\frac{1}{4}} e^{-\frac{\pi u}{2K'}} \frac{\Phi(u+K)}{\Phi(u)},$$

where $$r = e^{-\frac{\pi K}{K'}}$$

$\Phi(u)$ is, in fact, connected with Jacobi's $\Theta(u)$ by the equation $\Phi(u) \div \Phi(0) = e^{-\frac{\pi u^2}{4KK'}} \Theta(u) \div \Theta(0)$.

3. *Expansion of $\Phi(u)$ in a Hyper-harmonic Series containing odd Multiples of $\frac{\pi u}{2K'}$.*

From the above materials it is found that—

$$\Phi(u) = 2\left\{ \sqrt[4]{r} \cosh \frac{\pi u}{2K'} + \sqrt[4]{r^9} \cosh \frac{3\pi u}{2K'} + \sqrt[4]{r^{25}} \cosh \frac{5\pi u}{2K'} + \ldots \textit{ad infin.} \right\},$$

where $$r = e^{-\frac{\pi K}{K'}} \text{ and } \cosh x = \tfrac{1}{2}(e^x + e^{-x}).$$

4. *Some Consequences of the above Theorems.*

Among the numerous results which flow from the above I notice the following, viz.:—

$$(\alpha.)\quad a - \frac{J'}{K'} u = \frac{\pi}{2K'} \frac{\sqrt[4]{r} \sinh \frac{\pi u}{2K'} + 3\sqrt[4]{r^9} \sinh \frac{3\pi u}{2K'} + \ldots}{\sqrt[4]{r} \cosh \frac{\pi n}{2K'} + \sqrt[4]{r^9} \cosh \frac{3\pi u}{2K'} + \ldots}.$$

If this be combined with Jacobi's

$$Z(u) = a - \frac{E}{K} u = \frac{2\pi}{K} \left\{ \frac{q}{1-q^2} \sin \frac{\pi u}{K} + \ldots \right\}$$

we have the curious relation—

$$*\frac{u}{KK'} = \frac{1}{K'} \frac{\sqrt[4]{r} \sinh \frac{\pi u}{2K'} + 3\sqrt[4]{r^9} \sinh \frac{3\pi u}{2K'} + 5\sqrt[4]{r^{25}} \sinh \frac{5\pi u}{2K'} + \ldots}{\sqrt[4]{r} \cosh \frac{\pi u}{2K'} + \sqrt[4]{r^9} \cosh \frac{3\pi u}{2K'} + \sqrt[4]{r^{25}} \cosh \frac{5\pi n}{2K'} + \ldots}$$

* Other relations follow from the z function $z(u) = a - \frac{E + iJ'}{K + iK'} u$, which deserves to be studied. As regards the *transformation* of the function Φ, the results are very similar to those obtained in the case of Jacobi's Θ.—April 29, 1885.

$$-\frac{4}{K}\left\{\frac{q}{1-q^2}\sin\frac{\pi u}{K}+\frac{q^2}{1-q^4}\sin\frac{2\pi u}{K}+\frac{q^3}{1-q^6}\sin\frac{3\pi u}{K}+\ \ldots\right\}.$$

(β.) Putting $u=2n\mathrm{K}$, we deduce a simple identity, viz.:—
If n be an integer, then—

$$(1+2n)r^{n}+(3+2n)r^{2+3n}+(5+2n)r^{6+5n}+(7+2n)r^{12+7n}+\ \ldots\ ad\ infin.$$
$$=(1-2n)r^{-n}+(3-2n)r^{2-3n}+(5-2n)r^{6-5n}+(7-2n)r^{12-7n}+\ \ldots\ ad\ infin.$$

(γ.) From the formula $\mathrm{dn}u=\sqrt{k'}r^{\frac{1}{4}}e^{-\frac{\pi u}{2\mathrm{K}'}}\dfrac{\Phi(u+\mathrm{K})}{\Phi(u)}$,

we have
$$\sqrt{k'}=\frac{\Phi(0)}{r^{\frac{1}{4}}\Phi(\mathrm{K})}$$
$$=\frac{\{2\sqrt[4]{r}+\sqrt[4]{r^9}+\sqrt[4]{r^{25}}+\sqrt[4]{r^{49}}+\ \ldots\}}{r^{\frac{1}{4}}(r^{\frac{1}{4}}+r^{-\frac{1}{4}})+r^{\frac{9}{4}}(r^{\frac{3}{4}}+r^{-\frac{3}{4}})+r^{\frac{25}{4}}(r^{\frac{5}{4}}+r^{-\frac{5}{4}})+\ \ldots}$$
$$=2\ \frac{\sqrt[4]{r}+\sqrt[4]{r^9}+\sqrt[4]{r^{25}}+\sqrt[4]{r^{49}}+\ \ldots}{1+2r+2r^4+2r^9+2r^{16}+\ \ldots}$$

This result is, in fact, Jacobi's

$\sqrt{k}=2\dfrac{\sqrt[4]{q}+\sqrt[4]{q^9}+\sqrt[4]{q^{25}}+\ \ldots}{1+2q+2q^4+\ \ldots}$, as we can see by changing k' into k, and consequently r into q.

(δ.) From $\Phi(u)=\sqrt{\dfrac{2k'\mathrm{K}'}{\pi}}e^{\int_0^{z(u)du}}$ we deduce

$$\sqrt{\frac{2k'\mathrm{K}'}{\pi}}=\Phi(0)$$
$$=2\{\sqrt[4]{r}+\sqrt[4]{r^9}+\sqrt[4]{r^{25}}+\sqrt[4]{r^{49}}+\ \ldots\}$$
$$i.e.,\quad \sqrt{\frac{2\mathrm{K}'}{\pi}}=1+2r+2r^4+2r^9+2r^{16}+\ \ldots$$

5. *Extension of the above Method to a $\zeta_1(u)$ Function connected with Elliptic Integrals of the Third Kind.*

In a former note by the present writer mention was made of a $\zeta(u)$ function of the form $\zeta(u)=\dfrac{\pi}{2(1-\mu)\Pi\mathrm{K}'}\left(p-\dfrac{\Pi}{\mathrm{K}}u\right)$,

where

$$p=\int_0^{\theta}\frac{d\theta}{(1+n\sin^2\theta)\sqrt{1-k^2\sin^2\theta}},\quad \Pi=\int_0^{\frac{\pi}{2}}\frac{d\theta}{(1+n\sin^2\theta)\sqrt{1-k^2\sin^2\theta}},$$

$$\mu=\frac{\mathrm{P}'}{\Pi'}\div\frac{\mathrm{K}'}{\mathrm{K}},\quad \mathrm{P}'=\mathrm{K}'-\frac{n}{1+n}\Pi',$$

$$\Pi' = \int_0^{\frac{\pi}{2}} \frac{d\theta}{(1+n'\sin^2\theta)\sqrt{1-k'^2\sin^2\theta}}, \quad n'(1+n) = -k'^2.$$

This is not exactly the form considered by Jacobi, but if we write $\frac{\Pi}{K} - 1 = \frac{\mathrm{tn}u_0}{\mathrm{dn}u_0}Z(u_0)$ and $n = -k^2 sn^2 u_0$ his result is equivalent to

$$\zeta(u) = \frac{1}{2u_0}\log\frac{\Theta(u+u_0)}{\Theta(u-u_0)}.$$

Connected with $\zeta(u)$ is a *second* function of the form

$$\zeta_1(u) = \frac{\pi}{2(1-\mu)\Pi K'}\left(p - \frac{P'}{K'}u\right).$$

This satisfies the relations

$$\left.\begin{array}{r} \zeta_1(u+2iK') = \zeta_1(u) \\ \zeta_1(u+2K) - \zeta_1(u) = \frac{\pi}{K'} \end{array}\right\},$$

and I find that it can be expressed in terms of $\Phi(u)$ by means of the equation $\zeta_1(u) = \frac{1}{2u_0}\log\frac{\Phi(u+u_0)}{\Phi(u-u_0)}$, where u_0 is the same constant as above.

It thus appears that $\Theta(u)$ and $\Phi(u)$ are connected with and supplement each other in a very remarkable manner.

For example, if we write $\zeta(u)$ and $\zeta_1(u)$ in the more convenient forms $\zeta(u, u_0)$, $\zeta_1(u, u_0)$, it follows that besides Jacobi's result, $u_0\zeta(u, u_0) = u\zeta(u_0, u)$, we have likewise the equivalent form $u_0\zeta_1(u, u_0) = u\zeta_1(u_0, u)$.

II. "Further Observations on Enterochlorophyll and Allied Pigments." By C. A. MACMUNN, M.A., M.D. Communicated by Professor M. FOSTER, Sec. R.S. Received April 21, 1885.

(Abstract.)

In a paper read before the Royal Society in 1883, I described the results of an examination of the so-called "bile" of invertebrates, and showed that the alcohol extracts of their liver or other appendage of the intestine answering to that organ, showed a spectrum so like that of vegetable chlorophyll, as to have led me to assume that no essential difference exists between the spectrum of enterochlorophyll and plant chlorophyll.

At that time I could not decide the points which are now considered: (1) Is enterochlorophyll due to the presence of symbiotic algæ? (2) If not, is it an *immediate* food product, and merely an instance of the intra-cellular digestion of food chlorophyll? (3) If it is not due to either of these sources, can it be proved that it is built up by the animal containing it? (4) In what points does it differ from plant chlorophyll and that of *Spongilla ?*

I believe I can prove that the first two questions can be answered in the negative, and that it is an animal product, and does differ to a slight extent from vegetable chlorophyll, and also from that of Spongilla.

This evidence is based on the result of spectroscopic examination, especially of the bands in the blue half of the spectrum, on the results obtained by saponifying the colouring matters, and on the morphological characters of the enterochlorophyll in the organs containing it.

With regard to chlorophyll itself, I mean the mixture of colouring matters obtainable on extracting green leaves of land plants with alcohol, or with alcohol and ether, I believe that of the six bands of such a solution, five correspond to those seen in a living leaf, and that Kraus is correct in saying that such bands can be seen in a leaf.

The first four bands appear to belong to the green constituent of the chlorophyll, the other two to the yellow. On comparing solutions of enterochlorophyll with those of plant chlorophyll, it is seen that the bands corresponding to V and VI* of the latter are replaced by one, or by two, occupying a somewhat different position. The enterochlorophyll of the following species is described: *Paludina vivipara*, *Limnæus stagnalis*, *Trochus ziziphinus*, *Trochus cinerarius*, *Littorina littorea*, *Patella vulgata*, *Helix pomatia*, *Solaster papposa*, and several specimens of *Uraster rubens*, &c. In all these, enterochlorophyll is present, and presents very uniform spectroscopic characters, and the same as those described in the case of the Molluscs, Echinoderms, and Arthropods referred to in my former paper. The bands of the spectra of their solutions have been measured in wave-lengths, and show a remarkable agreement. In some cases two bands placed closely together in red replace the single dominant band of chlorophyll, and in every instance the solutions possess a red fluorescence.

In consequence of Hansen having published the result of saponifying vegetable chlorophyll, and of his having succeeded in obtaining certain crystals, which he maintains are those of isolated "chlorophyll green" and "chlorophyll yellow," I was anxious to try the effect of saponifying enterochlorophyll. It was necessary, however, to repeat his experiments on plant chlorophyll before saponifying entero-

* Adopting Kraus's numbers.

chlorophyll. On doing this, I found that his statement to the effect that chlorophyll is not decomposed by such treatment, is not supported by the results obtained. The bands of solutions after saponifying occur in an entirely different position from those of bands of similar solutions before saponifying, but I found the method useful in enabling me to compare the results when enterochlorophyll and Spongilla chlorophyll are saponified with those obtained in the case of plant chlorophyll.

I could separate, in the case of plant chlorophyll, the constituents called by Hansen "chlorophyll green" and "chlorophyll yellow," by adopting his methods, and found that the soap on repeated extraction with petroleum ether, lost all the yellow colouring matter, and that the latter could be obtained in some cases in yellow needles,* the residue giving the colour reactions of Schwalbe and Capranica. The alcohol and ether extracts on the other hand contained Hansen's "chlorophyll green," and none of the yellow constituent, and answered, except for the position of its bands, to Hansen's description.

On applying the same method to the chlorophyll of *Spongilla*, a complete separation of the constituents could not be brought about, as it was only partial, and an examination of the solutions showed a total alteration of spectra.

In the case of enterochlorophyll, saponification also alters the pigment. In some cases I succeeded in separating the yellow from the green constituent; and from the enterochlorophyll of *Uraster rubens*, I obtained *crystals* of "chlorophyll green" and "chlorophyll yellow," the former crystallising in sphere crystals, showing a black cross with polarised light, the latter in yellow radiating needles. But in almost every case it was found impossible to separate the constituents, as the petroleum ether showed a band in red, and the alcohol and ether bands in the blue end of the spectrum.

The solid chlorophyll yellow, while agreeing with that of plants in its behaviour towards nitric and sulphuric acids, did not, however, show the same blue-green colour with iodine in iodide of potassium, as it generally became reddish or remained unchanged.

I agree with Hansen that the chlorophyll yellow of plants is a "lipochrome," and also that of enterochlorophyll; the lipochromes being a class of colouring matters—so-called by Krukenberg—which were formerly known by the name of "luteins." Under this name are also included allied pigments, such as carotin, zoonerythrin, Kühne's chromophans—obtained by him from the retina, egg-lutein, and other pigments, which all possess bands in the blue and violet, and are soluble in such solvents as alcohol, ether, chloroform, bisulphide of carbon, benzol, petroleum ether, &c. They all are coloured

* It is not yet clearly proved whether these yellow needles may not belong to a fatty acid whose crystals are stained by the yellow colouring matter.

blue-green to blue by nitric and sulphuric acids, and generally blue-green with iodine in iodide of potassium (in the solid state).

On isolation of the yellow constituent of enterochlorophyll by saponification and extraction with petroleum ether, I found that it generally showed only one band, or sometimes two, but these bands generally gave different measurements from those of plant chlorophyll.

To see whether symbiotic algæ were present in the organs yielding enterochlorophyll, I examined fresh frozen sections, or portions of the organ teased out in salt solution, but the results were negative. On steeping such preparations, first in alcohol, then in weak solution of caustic soda, and neutralising with acetic acid, and afterwards testing with a solution of iodine in iodide of potassium and with Schultze's fluid, I never obtained evidence of the presence of starch or cellulose. Hence, apart from the absence of symbiotic algæ under the microscope, this result negatives their presence and also that of food chlorophyll. The morphology of enterochlorophyll was studied in similar preparations, and on the whole it appears to be present dissolved in oil globules and in granules, both of them enclosed in the epithelium lining the liver tubes. It also occurs dissolved in the protoplasm of the liver cells, and these appearances vary slightly in different cases.

It would therefore appear that enterochlorophyll is built up in the organ containing it; that it is a chlorophyll, of which there are several in animals, and that it is composed of two constituents, of which one resembles closely the corresponding constituent of plant chlorophyll, while the other is generally slightly different, but that no *essential* difference exists between the respective pigments is proved by the fact that the constituents of both may be obtained crystallised in the same form.

In enterochlorophyll there is probably a more intimate union between the constituents than in plant chlorophyll.

All readings are reduced to wave-lengths, and the most important spectra mapped in the accompanying charts. The appearance of enterochlorophyll under the microscope in different cases is also shown in the accompanying drawing, as well as the crystals referred to above.

III. "Note on a Previous Paper." By G. H. DARWIN, F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received March 19, 1885.

The paper entitled "On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains" ("Phil. Trans.,"

Part I, 1882, p. 187) has been found to be erroneous in certain points. The errors, however, do not touch the physical conclusions there attained. As this note has importance only in connexion with the paper, I proceed in the form of an appendix, without explanation of the notation.

In the first place—

Throughout the paper the normal stresses P, Q, R require an additional term W_i. The only function of these stresses used in obtaining physical results is P—R, and it remains unchanged when this correction is made.

The error takes its origin in § 1. Thomson's solution (1) when reduced to the form applicable to the incompressible solid, is the solution of the equations $-\frac{dp}{dx}+v\nabla^2\alpha=\frac{dW}{dx}$, and two others. The solution required is that of $-\frac{dp}{dx}+v\nabla^2\alpha=0$, and two others. The W involved in my solution is not the potential of a true bodily force, but only an "effective potential" producing the same strains as those due to the weight of the continents and mountains, but causing a different hydrostatic pressure. When, therefore, p is determined from Thomson's solution, that p is really equal to $p+W_i$ of the problem of the continents. Hence equation (3) should be $p=-\left(1+\frac{i}{I}\right)W_i$, instead of $p=-\frac{i}{I}W_i$. The correction to (3) must be carried on through the rest of the paper, and obviously it merely adds W_i to the stresses P, Q, R, leaving P—R, P—Q, Q—R unchanged.

The error would have been avoided had I, as suggested on p. 190, worked directly from the equations of equilibrium of the elastic incompressible solid, instead of from Thomson's solution.

When the solid is compressible, this method of "effective potential" (see "The Tides of a Viscous Spheroid," "Phil. Trans.," Part I, 1879, pp. 7–9) for including all the effects of gravitation is not applicable without certain additional terms in α, β, γ. Hence in § 10 where the solid is treated as being compressible the expressions for the stresses are incomplete. It will be found, however, that this incompleteness does not extend to the case of the mountains and valleys on the mean level surface, and that portion of the section remains correct. It would not be difficult to make the requisite corrections to the earlier part of the section, but I do not think it worth while to do so.

In the second place—

On p. 191 the following passage occurs:—

"It may be seen from considerations of symmetry that if W_i be a zonal harmonic, two of the principal stress-axes lie in a meridional

plane, and the third is perpendicular thereto. Moreover the greatest and least stress-axes are those which lie in that plane."

And in a foot-note on p. 200—

"It is easy to see that if a viscous sphere be deformed into the shape of a zonal harmonic, the flow of the fluid must be meridional, and from this we may conclude that in the elastic sphere the plane of greatest and least principal stresses must be also meridional. This has already been assumed to be the case in the present paper."

As one of the examiners for the Smith's Prizes at Cambridge, I have had placed before me an essay by Mr. Charles Chree, of King's College, in which he considers, amongst other points, the difference of principal stresses in an elastic sphere strained under the influence of the forces due to a potential expressed by the second zonal harmonic. In this essay Mr. Chree has pointed out that the conclusion thus arrived at by general reasoning is erroneous. His analytical treatment of the problem is entirely different from mine, and I cannot, therefore, avail myself of his actual work in amending the error which he has pointed out and corrected.

It is clear that, in the limiting case of the zonal harmonic where the surface becomes a series of parallel mountains and valleys on a flat surface, the principal stress parallel to the mountains must be zero, and the above reasoning has led to a correct conclusion.

But in the case of the second zonal harmonic, with either excess or deficiency of matter at the pole, there is a tendency for the equatorial regions to be either squeezed out or crushed in. Now an outward squeeze necessitates that the greatest pressure shall be perpendicular to the meridian, and this is contrary to the general conclusion quoted above. My error lay in overlooking this outward or inward tendency in the equatorial matter.

The conclusion is therefore wholly right in the case of the mountains and valleys, and at least partially wrong in the case of spheroidal deformation of the globe.

The data for examining into this question rigorously are given in my paper, and the best way of treating the matter is to rewrite § 5 on—

The State of Stress due to Ellipticity of Figure or to Tide-generating Forces.

When the effective disturbing potential W_i is a solid harmonic of the second degree, the solution found will give the stresses caused by oblateness or prolateness of the spheroid. It will also serve for the case of a rotating spheroid with more or less oblateness than is appropriate for the equilibrium figure. When an elastic sphere is under the action of tide-generating forces, the disturbing potential

is a solid harmonic of the second degree, and therefore the present solution will apply to this case also.

If we extract the case $i=2$ from Tables I, II, III, and put $i=2$ in (26), and substitute colatitude θ for latitude l, we have after some simple reductions—

$$\left.\begin{aligned} 19(\mathrm{P}-W_2) &= 16a^2-(19+3\cos 2\theta)r^2 \\ 19(\mathrm{R}-W_2) &= -32a^2+(29+3\cos 2\theta)r^2 \\ 19(\mathrm{Q}-W_2) &= 16a^2-(13+9\cos 2\theta)r^2 \\ 19\mathrm{T} &= 3\sin 2\theta r^2 \end{aligned}\right\} \quad . \quad . \quad . \quad (a).$$

[Note the introduction of W_2 in the P, Q, R, in accordance with the first correction.]

Let N_1, N_2, N_3, be the three principal stresses, each diminished by W_2, so that—

$$\left.\begin{aligned} &\left.\begin{aligned}\mathrm{N}_1+W_2 \\ \mathrm{N}_3+W_2\end{aligned}\right\} = \tfrac{1}{2}(\mathrm{P}+\mathrm{R})\pm\tfrac{1}{2}\sqrt{\{(\mathrm{P}-\mathrm{R})^2+4\mathrm{T}^2\}} \\ &\mathrm{N}_2+W_2 \quad = \mathrm{Q} \end{aligned}\right\} \quad . \quad . \quad . \quad (b).$$

Then—

$$\left.\begin{aligned} &\left.\begin{aligned}19\mathrm{N}_1 \\ 19\mathrm{N}_3\end{aligned}\right\} = -8a^2+5r^2\pm 3\sqrt{\{64(a^2-r^2)^2+r^4-16r^2(a^2-r^2)\cos 2\theta\}} \\ &19\mathrm{N}_2 \quad = 16a^2-13r^2-9r^2\cos 2\theta \end{aligned}\right\} \quad (c).$$

Now let us find the surfaces, if any, over which $\mathrm{N}_2=\mathrm{N}_1$ or N_3. They are obviously given by—

$$24a^2-18r^2-9r^2\cos 2\theta = \pm 3\sqrt{\{64(a^2-r^2)^2+ \quad . \quad . \quad \&c.\}}.$$

This easily reduces to—

$$r^2(1-\cos 2\theta)[32a^2-20r^2-9r^2(1+\cos 2\theta)]=0 \quad . \quad . \quad (d).$$

Thus the solutions are—

$$\left.\begin{aligned} r&=0 \\ \theta&=0 \text{ and } \pi \\ \text{and } 32a^2-20(x^2+y^2)-38z^2&=0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (e).$$

By trial it is easy to see that at the centre and all along the polar axis $\mathrm{N}_2=\mathrm{N}_1$, and that inside of the ellipsoid $10(x^2+y^2)+19z^2=16a^2$, N_2 is greater than N_1, and outside it is less.

Hence inside of the ellipsoid $\mathrm{N}_2-\mathrm{N}_3$ and outside of it $\mathrm{N}_1-\mathrm{N}_3$ is the stress-difference. $\mathrm{N}_2-\mathrm{N}_3$ nowhere vanishes so long as N_2 is not equal to N_1, and $\mathrm{N}_1-\mathrm{N}_3$ vanishes where $r=\frac{2}{3}\sqrt{2}\,.\,a=\cdot 9428a$ and $\theta=0$, which is inside of the region for which $\mathrm{N}_1-\mathrm{N}_3$ is the stress-difference. This is the only point in the whole sphere for which the stress-difference vanishes.

The ellipsoid of separation cuts the sphere in colatitude 35° 16′.

If we put Δ for stress-difference, then between the centre and the ellipsoid—

$$19\Delta = 24a^2 - 18r^2 - 9r^2\cos 2\theta + 3\sqrt{\{64(a^2-r^2)^2 + r^4 - 16(a^2-r^2)r^2\cos 2\theta\}} \quad . \quad . \quad . \quad (f),$$

and between the polar surface regions and the ellipsoid—

$$19\Delta = 6\sqrt{\{64(a^2-r^2)^2 + r^4 - 16(a^2-r^2)r^2\cos 2\theta\}} \quad . \quad . \quad (g).$$

This last also holds for the whole polar axis, along which—

$$19\Delta = 6(8a^2 - 9r^2) \text{ or } 6(9r^2 - 8a^2).$$

[In the paper the form (g) for Δ was taken as applicable to the whole sphere; the maximum value of Δ arises from the form (g), and was therefore correctly computed.]

In order to find the actual value of Δ in any special case, we have to multiply the expression for Δ by appropriate factors, determined in the paper. For the present it will be convenient to omit these factors.

We may now from (f) and (g) determine the distribution of stress-difference throughout the sphere.

By computation and graphical interpolation I have drawn the annexed figure, showing the curves of equal stress-difference through-

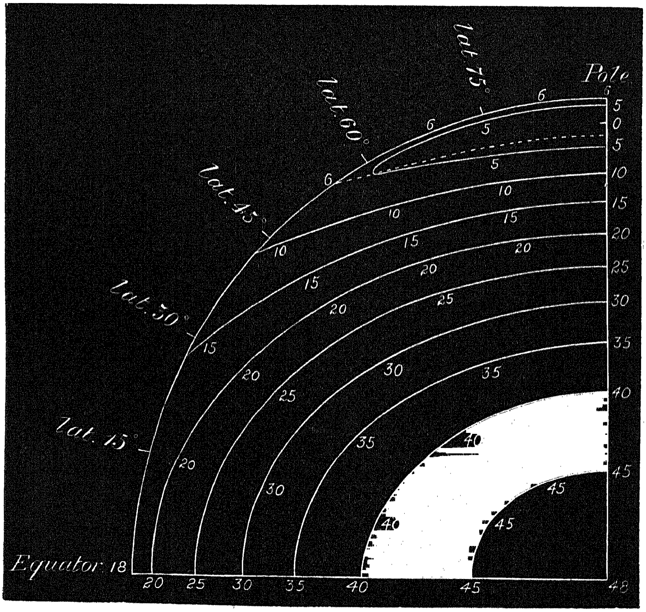


Diagram showing curves of equal Stress-difference due to the weight of 2nd harmonic inequalities or to tide-generating force.

out a meridional section of the sphere. The numbers written on the curves give the values of 19Δ, when the radius of the sphere is unity. The point marked 0 is that where Δ vanishes.

The dotted curve is the ellipse of separation (e) cutting the circle in colatitude 35° 16′.

Over the polar cap and at the surface 19Δ is constant and equal to 6; at the surface from colat. 35° 16′ to the equator 19Δ increases from 6 to 18, varying as the square of the sine of the colatitude.

At the centre 19Δ is 48, being eight times the polar superficial value.

Beginning with the first sentence of p. 203 the remainder of § 5 will hold good. It is well to observe, however, that where surface stress-difference is spoken of, it must be taken as referring to the polar caps only, the stress-difference at the equator being three times as great. It is worth while comparing the figure 1 of the paper (Plate 19) with the figure now given.

We now come to the case of—

The Stresses due to the even Zonal Harmonics.

The complete determination of the regions within which N_2-N_3 and N_1-N_3 are the proper measures of stress-difference might be somewhat difficult. As, however, these harmonics are only used for the determination of stress-difference in the equatorial regions, it is sufficient to find the boundary of the regions for that part of the sphere.

We see from (22) that $\sqrt{\{(P-R)^2+4T^2\}}$ only differs from $P-R$ by terms which depend on the square of the sine of the latitude.

Hence as far as the first power of $\sin l$ we have

$$N_1=P-W_i,\quad N_2=Q-W_i,\quad N_3=R-W_i.$$

Therefore if we neglect terms depending on the square of the sine of the latitude, we have from (22),

$$\frac{IN_1}{r^{i-2}}=A_0r^2+B_0a^2,\quad \frac{IN_2}{r^{i-2}}=G_0r^2+H_0a^2,\quad \frac{IN_3}{r^{i-2}}=C_0r^2+D_0a^2.$$

Then substituting, for A_0, B_0, &c., their values from (23), (24), (26), and effecting some easy reductions, we find,

$$\frac{IN_1}{r^{i-2}}=\quad i^2(i+2)(a^2-r^2).$$

$$\frac{IN_2}{r^{i-2}}=\quad i^2(a^2-r^2)+\frac{3i^2}{i-1}a^2.$$

$$\frac{IN_3}{r^{i-2}}=-[i(i+1)(i+2)+i](a^2-r^2)-\frac{i(i^2+3)}{i-1}a^2.$$

From this we see that N_1 is always positive but vanishes at the surface, N_2 is always positive but does not vanish at the surface, and N_3 is always negative.

Hence at the surface and for some distance beneath it, the stress-difference is N_2-N_3; but below the surface at which N_1 becomes equal to N_2, we have N_1-N_3 as the stress-difference.

This surface is determined by

$$i^2(i+2)(a^2-r^2)=i^2(a^2-r^2)+\frac{3i^2}{i-1}a^2.$$

whence

$$\frac{r^2}{a^2}=\frac{i^2-4}{i^2-1}$$

Solving for the successive even values of i, we find, when

$i=2$, $\frac{r}{a}=0$, as we already know.

$i=4$, $\frac{r}{a}=0{\cdot}8944$,

$i=6$, $\frac{r}{a}=0{\cdot}9562$,

$i=8$, $\frac{r}{a}=0{\cdot}9759$,

$i=10$, $\frac{r}{a}=0{\cdot}9847$.

In the paper N_1-N_3 was always taken as being the stress-difference, and we now see that even when $i=4$, the region is very thin in which this is untrue and where N_2-N_3 is the proper measure. For the higher harmonics it soon becomes negligeable.

This explains the transition from the incorrectness of the treatment in the paper of the case of the second harmonic to the correctness of the treatment of the mountain ranges.

On looking at § 7 and the accompanying figures we see that the maximum stress-difference occurs far within the region within which N_2 becomes the mean principal stress. Thus § 7 may be permitted to stand, save that in fig. 4, Plate 19, the ordinates of the curves $i=4$, $i=6$, &c., are to be slightly augmented at the surface where $r=a$. It is easy to see what small alterations are to be made in Table VI, and in the subsequent discussion, but clearly nothing material from a physical point of view need be amended.

It may be remarked in conclusion that, whilst it is proper to correct the mathematical errors in this paper, the physical conclusions remain untouched.

May 7, 1885.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Baird, A. W., Major, R.E.
Carpenter, Philip Herbert, D.Sc.
Clark, Sir Andrew, Bart., M.D.
Common, Andrew Ainslie, F.R.A.S.
Creak, Ettrick William, Staff Commander, R.N.
Divers, Prof. Edward.
Hicks, Henry, M.D.
Hicks, Prof. William Mitchison, M.A.
Japp, F. R., Ph.D.
Marshall, Prof. Arthur Milnes, M.D.
Martin, Prof. Henry Newell, D.Sc.
O'Sullivan, Cornelius.
Perry, Prof. John.
Ringer, Prof. Sydney.
Vines, Sidney Howard, D.Sc.

The following Papers were read:—

I. "A Study of the Thermal Properties of Ethyl Alcohol." By WILLIAM RAMSAY, Ph.D., and SYDNEY YOUNG, D.Sc. Communicated by Professor G. G. STOKES, Sec. R.S. Received April 18, 1885.

(Abstract.)

The abnormal vapour-density of many compounds has been ascribed to their dissociating to a greater or less degree while in the gaseous state. The compound molecule yields, with increase of temperature, a constantly increasing amount of those simpler molecules into which it dissociates; and as this dissociation is attended with increase of volume, the vapour-density of the mixture of gaseous molecules decreases with rise of temperature.

But this phenomenon is not confined to dissociating compounds alone. It is known that many, if not all, liquids acquire an abnormal vapour-density in proximity to their point of saturation. In studying

the dissociation of a compound, observations regarding its vapour-density, which would apparently lead to a correct estimate of the amount of dissociation under any given constant temperature and pressure, are complicated by the phenomena exhibited by liquids as such; and it still admits of question whether bodies with such abnormal vapour-densities as are displayed by acetic and homologous acids, should have their abnormality ascribed to dissociation; or, less exclusively, it may be asked, how much of this abnormality is to be attributed to the one cause and how much to the other?

To reach a conclusion on this point, it was necessary to study and compare the behaviour of substances belonging to the four types:—(*a*) liquids, the vapours of which are not known to dissociate under the conditions of temperature of the experiment; (*b*) liquids, the vapours of which probably dissociate into like molecules; (*c*) bodies which dissociate gradually in the gaseous state into unlike molecules; (*d*) bodies which dissociate completely on passage into the gaseous state.

The liquid ethyl alcohol has been chosen as a typical representative of the first class, and its behaviour has been fully studied. Numerous measurements have been made which establish relations:—(*a*) between volume of liquid alcohol and temperature at various pressures; (*b*) between volume of liquid alcohol and pressure (compressibility) at various temperatures; (*c*) between volume of unsaturated and of saturated vapour, temperature, and pressure; and (*d*) the heats of volatilisation have been calculated from these data. The limits of temperature extended from 13° to 246°; and the limits of pressure from 10 mm. to 60,000 mm.

The point of chemical importance deduced from this research is that alcohol vapour in contact with liquid acquires its normal density, 23, at about 50°; and that at lower temperatures no tendency towards a rise in vapour-density could be detected; as it will be shown in a subsequent memoir that the vapour of acetic acid, in contact with its liquid, acquires increased density on lowering temperature and pressure, the probable conclusion may be drawn that complex molecular groups are produced in larger number, or exhibiting greater complexity with decrease of temperature. The vapour of alcohol, on the other hand, shows no such tendency.

This research has also shown that the critical point of ethyl alcohol lies at a temperature differing not more than 0·5° from 243·6°, and at a pressure of nearly 48,900 mm., while the volume of 1 gram of the critical fluid is approximately 3·5 c.c.

II. On the Solubility of Calcium Sulphate in Water in the presence of Chlorides." By WILLIAM A. TILDEN, D.Sc., F.R.S., Professor of Chemistry in the Mason Science College, Birmingham, and W. A. SHENSTONE, F.I.C., F.C.S., Lecturer on Chemistry, Clifton College, Bristol. Received April 27, 1885.

It has long been known* that the solubility of calcium sulphate in water attains a maximum at about 35° C., also that the solubility of this compound is increased by the presence of common salt. But accurate determinations of the extent to which the solubility is thus influenced have not hitherto been published. Having the means of determining solubilities at temperatures above the boiling point of water, and considering the interest from both a theoretical and practical point of view attaching to these determinations, we undertook the following experiments. When our work was nearly completed we observed a paper by Professor Lunge on the same subject in the Journal of the Society of Chemical Industry for January last. We venture to think, however, that our results are still of interest as being more systematic, and since every experiment was done with every precaution and very great care, they are probably more accurate. The solubilities at temperatures above 100° have never before been determined.

Our former experiments on solubilities at high temperatures† were made in a gun-metal tube coated with silver, but in the present series of experiments the silver was not found to resist the action of the solutions, and we have therefore employed a tube of platinum. The platinum tube is shown in the accompanying figure.

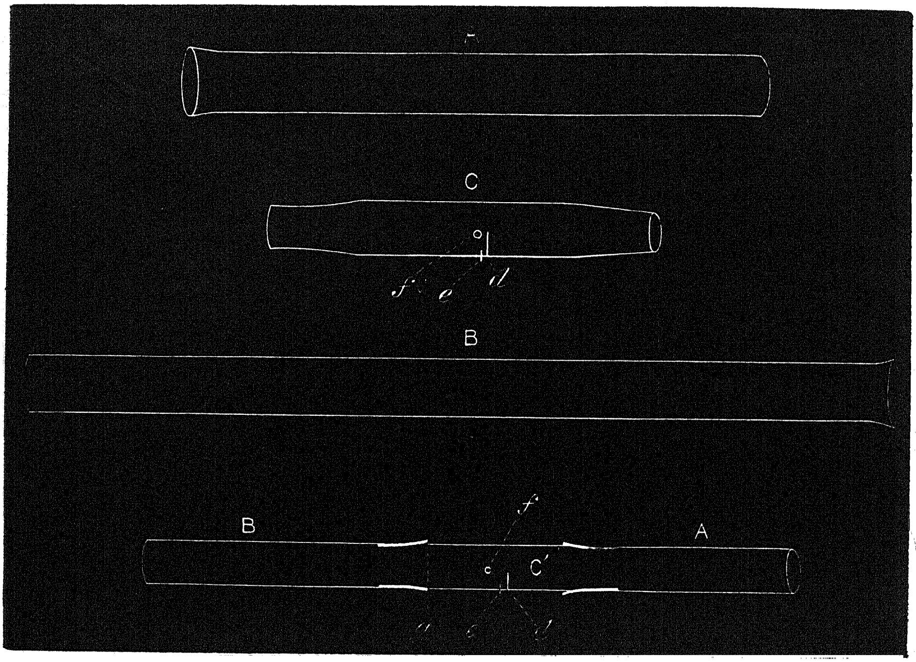
The part A is 87 mm., B 123 mm. long. Both are closed at one end, and the open ends are ground to fit the union piece C, which contains a small screen *d*, and has two small holes at *e* and *f*. The internal diameter of the arrangement is 12 mm. When joined together, the three pieces form a compound tube C′, 245 mm. long.

The salt and solvent are placed in B, a small strainer of platinum gauze is introduced at *g*, and the tubes are fitted together. The whole is then enclosed in the strong gun-metal tube and heated in the paraffin bath as already described ("Phil. Trans."). After four or five hours at the desired temperature the solution is drained into A, and after turning the tube so that the screen may arrest the flow of any subsequent drainings into A, the whole is allowed to cool. The small hole at *e* allows any drops of liquid that may be stopped by the screen to escape into the outer tube.

* Poggiale, "Ann. Chim. Phys." [3], viii, 469.

† "Phil. Trans." 1884, I, 23.

FIG. 1.



In the experiments made at lower temperatures the solutions were placed together with excess of pure calcium sulphate in stoppered bottles or flasks, which were immersed in the bath maintained at a constant temperature for at least five hours with constant agitation. At temperatures above 20° a special contrivance was used for filtering off the solution required for analysis without change of temperature or evaporation.

All the materials employed were pure, and filters purified by acid were alone used.

Two series of determinations have been made, the first with a solution containing a fixed proportion of chloride at different temperatures, and the second at constant temperature with a varying strength of solution.

These results are indicated graphically on the accompanying diagram (fig. 2), from which it will be observed that whilst the solubility of calcium sulphate in water is increased by the addition of chloride of sodium or of ammonium, probably owing to double decomposition, it is diminished by the presence of chloride of calcium. It may also be noticed, however, that the form of the curve is nearly the same in

each case, the solubility being greatly diminished at temperatures above 100°.

Table I.—Solubility of Calcium Sulphate in a solution of Common Salt at various Temperatures.

Temperature.	Parts of NaCl to 100 parts water.	Parts of $CaSO_4$ to 100 parts water.
20°	19·90	·823*
44	19·93	·830
67	19·95	·832
85	19·90	·823
101	20·08	·682
130	19·92	·392
165	20·04	·250
169	20·05	·244
179	20·10	·229
225	21·00	·178

Table II.—Solubility of Calcium Sulphate in a solution of Ammonium Chloride at various Temperatures.

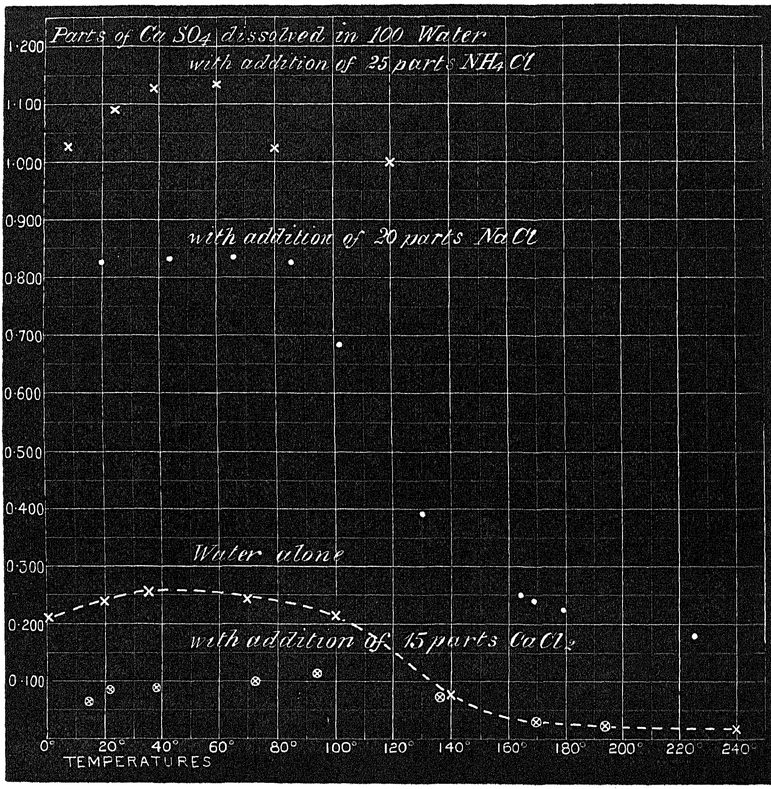
Temperature.	Parts of NH_4Cl to 100 water.	Parts of $CaSO_4$ to 100 water.		
		Exp. 1.	Exp. 2.	Mean.
8°	25	1·061	·999	1·030
9	25	1·011	1·036	1·023
25	25	1·095	1·097	1·096
39	25	1·133	1·120	1·126
60	25	1·121	1·146	1·133
80	25	1·024	1·029	1·026
120	25	1·000	..	1·000

Table III.—Solubility of Calcium Sulphate in solution of Calcium Chloride at various Temperatures.

Temperature.	Parts of $CaCl_2$ to 100 parts water.	Parts of $CaSO_4$ to 100 parts water.
15°	15·00	·063
21	14·70	·086
39	15·00	·091
72	14·90	·100
94	15·16	·110
138	14·70	·071
170	14·82	·031
195	14·70	·022

* Mean of two experiments.

FIG. 2.



An attempt was made to ascertain the influence of magnesium chloride. A solution was made by dissolving pure anhydrous chloride of magnesium in water, and the calcium sulphate was digested with this solution at known temperatures.

Table IV.—Solubility of Calcium Sulphate in solution of Magnesium Chloride at various Temperatures.

	100 parts of water contain			
	Experiment 1.		Experiment 2.	
Temperature.	$CaSO_4$.	$MgCl_2$.	$CaSO_4$.	$MgCl_2$.
9°	·765	19·7	·778	19·9
39	2·744	11·1	2·747	not deter.
80	1·038	9·99	1·038	9·99

From this it will be seen that whilst the magnesium chloride is rapidly decomposed and precipitated by the water as the temperature is raised, yet the solvent action of the hydrochloric acid which is thus formed in the solution is partly counteracted, probably by the magnesium chloride which remains dissolved, and which may act in the same manner as calcium chloride.

Table V.—Solubility of Calcium Sulphate in Water in presence of various proportions of Sodium Chloride. Temperature 20°.

100 parts of water	
Containing parts of NaCl.	Dissolve parts of $CaSO_4$.
0 ·00	·225
0 ·52	·301
2 ·03	·441
5 ·02	·615
5 ·05	·634
10 ·00	·738
20 ·00	·823
24 ·40	·820
35 ·10	·734
35 ·86	·709

FIG. 3.

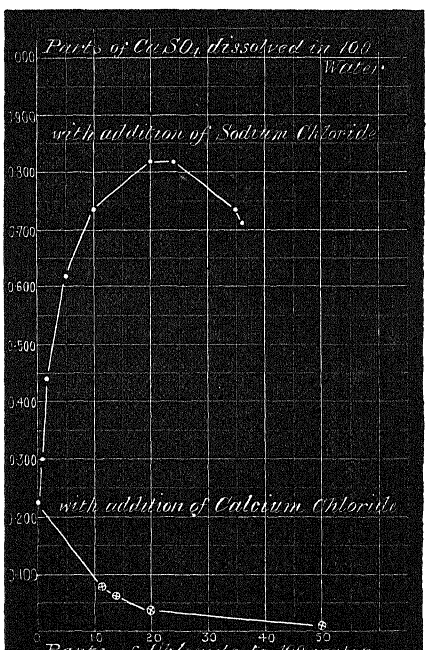


Table VI.—Solubility of Calcium Sulphate in Water in presence of various proportions of Calcium Chloride. Temperature 20°.

Parts of $CaCl_2$ in 100 water.	Parts of $CaSO_4$ in 100 water.
0·00	·225
11·50	·078
14·39	·063
19·80	·041
51·00	·000
67·05	·000

The addition of common salt, therefore, increases the solubility of calcium sulphate until the proportion of salt to water is 20 or 25 to 100. A larger quantity diminishes the solubility. Calcium chloride added to solution of calcium sulphate causes steady decrease of solubility, till when the proportion amounts to about 50 to 100 water the sulphate is practically insoluble. These results are very plainly observable in the accompanying curve (Fig. 3).

III. "Contributions to the Chemistry of Chlorophyll. Part I." By EDWARD SCHUNCK, F.R.S. Received April 30, 1885.

(Abstract.)

The paper treats of the products formed by the action of acids on chlorophyll. All who have worked with chlorophyll are familiar with the peculiar effects produced in solutions of chlorophyll by the action of acids. The colour is changed, and an absorption spectrum makes its appearance which differs from that of chlorophyll. According to some, these changes are due to a simple modification of the chlorophyll, others consider they are caused by the formation of products of decomposition. The latter view seems the more probable.

On passing a current of hydrochloric acid gas into an alcoholic solution of chlorophyll, a dark green, almost black, precipitate is formed at once. The greenish-yellow liquid contains substances extracted along with chlorophyll by the alcohol, and not connected with the latter. The precipitate consists essentially of two colouring matters, phyllocyanin and phylloxanthin, bodies that had been previously observed and so named by Fremy, who however did not obtain them in a state of purity. They are best separated by Fremy's method, which consists in dissolving the mixture in ether and then adding concentrated hydrochloric acid, when the liquid separates into two layers, a lower blue one containing phyllocyanin, and an upper yellowish-green one containing phylloxanthin. It is immaterial what

kind of leaves are taken for extraction, the products are always the same.

The paper deals only with the properties of phyllocyanin, which are very peculiar. After being purified in the manner described, it is obtained as a dark blue mass resembling indigo, and consisting of microscopic crystals which are generally opaque, but sometimes when very thin are translucent, and then appear olive-coloured. It stands heating to 160° without decomposition, but between that temperature and 180° it is decomposed without previously fusing, leaving a charred mass which on further heating burns away without residue. It contains nitrogen, but is free from sulphur.

Phyllocyanin is insoluble in water, petroleum ether, and ligroin, but dissolves in alcohol, ether, chloroform, glacial acetic acid, benzol, aniline, and carbon disulphide. The best solvent is chloroform. A minute quantity of the substance imparts an intense colour to any one of these solvents. It is only on diluting largely that the solutions lose their opacity. They then appear of a dull green or olive colour, and show the well-known and often described spectrum of so-called "acid chlorophyll," consisting of five bands, three of which are very dark, one of moderate intensity, and the fifth very faint.

By oxidising agents, such as nitric or chromic acid, phyllocyanin is easily decomposed, yielding yellow amorphous products, the solutions of which show no absorption bands. It shows a remarkable degree of permanence as compared with chlorophyll, when exposed to the combined action of air and light. A chloroformic solution contained in a loosely stoppered bottle may be exposed for weeks or even months to alternate sunlight and diffused daylight before its peculiar colour and all trace of absorption bands have disappeared. When the process is complete a yellow liquid results, which contains several products, all of them amorphous, one being easily soluble in water and exceedingly bitter to the taste. The decoloration of a chlorophyll solution under the same circumstances would take place in a day or two.

A small quantity of bromine added to a chloroformic solution of phyllocyanin changes the colour to a bright grass-green, which exactly resembles that of a chlorophyll solution. By a further addition of bromine, the solution loses its green colour and acquires a reddish hue; it now shows the same spectrum as a phyllocyanin solution; but the bands are all shifted towards the red end. An excess of bromine produces no further change. On passing chlorine gas through the solution in chloroform, it passes through the same phases of colour, first becoming grass-green, then reddish, but a further change takes place on continuing to pass chlorine through the solution, which now becomes yellow, finally pale yellow, all traces of absorption bands at the same time disappearing. The products both with bromine and chlorine are amorphous.

Phyllocyanin dissolves easily in concentrated sulphuric, hydrochloric, and hydrobromic acids, yielding dark blue solutions, which show spectra differing from that of phyllocyanin, and no doubt contain compounds of the latter with acids. These compounds however are unstable, for on the addition of water to the solutions, phyllocyanin is precipitated unchanged. Phyllocyanin shows no tendency to combine with weaker acids, such as phosphoric, oxalic, tartaric, or citric acid.

Phyllocyanin dissolves easily in dilute caustic potash or soda lye. The solution gives precipitates of various shades of green with earthy and metallic salts, such as barium chloride, calcium chloride, lead acetate, and cupric acetate, and these might be called phyllocyanates. It seems however that by mere solution in alkali, phyllocyanin undergoes some change, for if acetic acid in excess be added to the solution and it be then shaken up with ether, the precipitate dissolves in the ether, giving a solution which shows the bands of phyllocyanin, but if the whole be left to stand some time, the colour of the ethereal solution changes from green to brown, and it now shows a distinct and peculiar spectrum, characterised by two bands in the red and two fine but well-marked bands in the green, the third and fourth bands of phyllocyanin having disappeared, while the fifth still remains. The body yielding this spectrum has been prepared and found to yield microscopic crystals like phyllocyanin. A different product is formed when hot alkaline lye, or, what is better, boiling alcoholic potash or soda is employed. It crystallises in small rosettes, which are green by transmitted, of a fine purple by reflected light. Its solutions have a dull purple colour, and exhibit a distinct spectrum characterised by a broad very dark band in the green. It may be identical with one of the products obtained by Hoppe-Seyler from his chlorophyllan with alkalis.

The action of aniline on phyllocyanin is peculiar and interesting. When the two are heated together in a sealed tube to 130° the phyllocyanin disappears entirely, giving several products, one of which is colourless and crystallises in white needles. The second, which may be an anilide, yields solutions which are quite red, and show a characteristic spectrum having three fine but distinct bands in the red, and three other very strong bands, one in the yellow, one in the green, and one at the edge of the blue. No similar compound is formed when ammonia is used in place of aniline.

The concluding part of the paper treats of what may be called double compounds of phyllocyanin, into which metals and acids, especially organic acids, enter as constituents. Phyllocyanin seems to act the part of a weak base, uniting with strong acids and forming unstable compounds. In acetic acid it merely dissolves without yielding any compound. In like manner, when freshly precipitated

cupric oxide or zinc oxide is added to a solution of phyllocyanin in boiling alcohol no combination takes place. A very different effect is observed when either of the two oxides is employed along with acetic acid. When cupric oxide is added to a solution of phyllocyanin in boiling acetic acid the solution acquires at once a deep greenish-blue colour, and it no longer contains uncombined phyllocyanin, for its spectrum is different, and on standing it deposits lustrous crystals, which doubtless consist of a compound containing phyllocyanin, acetic acid, and copper. If zinc oxide be employed a similar effect is observed, the liquid acquires an intense green colour like that of a chlorophyll solution, and now contains the corresponding acetate of phyllocyanin and zinc. The same phenomenon is seen when ferrous oxide, manganese oxide, or silver oxide is taken, liquids of various shades of green being obtained which contain phyllocyanin compounds, but no similar compounds are formed when potassium, sodium, barium, calcium, magnesium, or lead acetate is employed. Acetic acid is, however, not the only acid which yields the reaction. If palmitic, stearic, oleic, tartaric, citric, malic, or phosphoric acid be employed, it takes place just as with acetic acid, but in some cases time is required for its completion. Oxalic acid, however, seems to be without effect, and tartaric acid fails in some cases.

The various compounds have a number of properties in common, though the several classes differ, *inter se*, in some important particulars. They all dissolve more or less readily in alcohol, ether, chloroform, benzol, and carbon disulphide, but are all insoluble in water with the exception of the phyllocyanin manganese acetate, which dissolves readily therein. The solutions have a green colour varying from grass-green, like that of chlorophyll solutions, to a fine bluish-green or blue, and they show peculiar spectra. The alcoholic solutions remain unchanged when sulphuretted hydrogen is passed through them, no precipitate is formed, and the solutions on evaporation leave the various compounds with their original properties. It is only on incineration that the presence of metallic constituents is detected. Lastly, they are all soluble in dilute alkaline lyes, and are reprecipitated unchanged on the addition of acetic acid. These reactions make it somewhat doubtful whether these compounds are to be considered as double salts in the ordinary acceptation, and whether the metallic constituents may not rather be contained in them somewhat in the same way as the iron in hematin. Of the various compounds those belonging to the cupric class are the most stable; they are not decomposed by boiling hydrochloric acid. The zinc compounds, on the other hand, are very readily decomposed by hydrochloric acid, yielding phyllocyanin.

The behaviour of phyllocyanin towards zinc oxide in the presence of acids may serve to explain a peculiar phenomenon first observed by

Professor Church, and subsequently described* by Tschirch. The former took chlorophyll that had become brown on standing, and acting on it with zinc powder obtained a body yielding green solutions, which he took to be regenerated chlorophyll. Tschirch acted on Hoppe-Seyler's chlorophyllan with zinc powder and observed the same phenomena, the conclusion at which he arrived being the same, viz., that chlorophyll is reproduced from chlorophyllan by reduction. It is probable, however, that what they obtained was in reality a zinc compound of phyllocyanin, and would have been formed just as well by using zinc oxide. Chlorophyllan is probably an impure substance containing some fatty acid along with phyllocyanin, so that by the action of zinc oxide it may yield a compound similar to those above mentioned. The experiment was tried with the crude product obtained by passing hydrochloric acid gas into a solution of chlorophyll. Some of this was dissolved in alcohol, and the solution was boiled with zinc oxide, when it gradually became of a bright green like a solution of chlorophyll, but its spectrum differed, being identical with that of the zinc compounds obtained directly from phyllocyanin.

IV. "On the Electric Resistance of a New Alloy named Platinoid." By J. T. Bottomley, M.A., F.R.S.E. Communicated by Sir W. Thomson. Received May 5, 1885. Read May 7, 1885.

In the course of a series of experiments on the electric resistance of various metals and alloys, and in particular on the variation of the electric resistance of these metals and alloys with temperature, I have examined a new alloy (called by the inventor platinoid), which has turned out to have important properties.

This alloy is the invention of Mr. F. W. Martino, of Sheffield; and I have to acknowledge my indebtedness to Mr. Martino for having provided me with specimens of his new alloy and given me information regarding it; and for having supplied me with wires specially drawn down to the finer gauges for my experiments.

The inventor, searching experimentally for a means of rendering tarnishable metals and alloys less tarnishable, had satisfied himself that the addition of pure metallic tungsten imparted greater density to alloys, and likewise less tendency to oxidation. Having found a mode of combining a small quantity of tungsten with copper, nickel, and zinc, he produced a white alloy resembling the alloys of silver, which proved very little tarnishable under atmospheric

influences. Accordingly he patented the process, and registered the alloy under the name platinoid.

Platinoid is practically German silver with the addition of a small percentage (1 or 2 per cent.) of metallic tungsten. The tungsten is added in the form of phosphide of tungsten, a considerable percentage of which is in the first place fused with a portion of the copper. The nickel is then added; and then the zinc and the remainder of the copper. The mixture requires to be re-fused more than once, and during the process the phosphorus and a considerable portion of the tungsten originally added is removed as scoriæ. In the end there is obtained a beautiful white alloy, which is platinoid. When polished the alloy is scarcely distinguishable in appearance from silver. To test the quality claimed for it as to being untarnishable, I have for some weeks been keeping ornamental specimens lying exposed to the ordinary town atmosphere; and I have satisfied myself that the alloy has a very remarkable power of resisting the tarnishing influence of the air of a large town.

It is, however, with the electric resistance of platinoid that I have chiefly interested myself. German silver wire has proved of great use in the construction of galvanometer coils and of resistance coils, on account of two important properties, viz., its very high resistance, and the smallness of the variation of its resistance with change of temperature. Both these properties are possessed in a still higher degree by platinoid alloy.

The resistance of German silver differs considerably in different specimens. It is commonly stated to be $21{\cdot}17 \times 10^{-6}$ B.A. ohms between opposite faces of a centimetre cube at 0°C.*; or, reducing to legal ohms, $20{\cdot}935 \times 10^{-6}$ legal ohms between the opposite faces of a centimetre cube. The table on page 342 shows the resistance of a number of specimens of platinoid wire.

It appears from these results that the specific resistance of platinoid is about one and a half times that of German silver.

The experiments on the variation of resistance of platinoid with temperature were carried on in the following way. The specimen of platinoid to be tested was wound on a wooden bobbin, on the surface of which a screw had been cut, and the spires of the helix were kept separate by lying between the threads of the screw. This coil was immersed in a bath of oil, and was connected in series with a known wire of German silver, the temperature of which was kept constant, and with a single Daniell's cell. The differences of potential between the two ends of the platinoid wire and the two ends of the German silver wire were determined by applying the electrodes of a high-resistance

* Given by Prof. Fleeming Jenkin, F.R.S., as expressing the results of Matthiessen's experiments.

Specifying number.	Diameter in decimals of a centimetre.	Cross section.	Resistance, legal ohms per metre.	Resistance between opposite faces of a centimetre cube, legal ohms.
16	·1610	·0204300	·181	$36{\cdot}98 \times 10^{-6}$
17	·1430	·0160200	·202	32·36
18	·1230	·0119400	·288	34·38
19	·1110	·0096770	·353	34·16
20	·0865	·0058760	·555	32·61
A	·0595	·0027810	1·250	$34{\cdot}76 \times 10^{-6}$
B	·0495	·0019240	1·707	32·85
28	·0402	·0012690	2·605	33·06
29	·0340	·0009070	3·412	30·94
32	·0290	·0006605	4·371	28·87
36	·0220	·0003801	8·219	31·24

galvanometer. The ratio of the differences of potential is the same as the ratio of the resistances of the two wires. This method of comparing the resistance of an unknown wire with that of a known wire gives admirable results, and I have recently made great use of it.

In the following table is shown the ratio of the resistances of a specimen of platinoid wire at different temperatures to its resistance at zero. The wire was the same as that specified as No. 20 in the table of resistances. The length of the wire experimented on was about four-fifths of a metre. The only trouble in the experiment was the keeping the oil-bath, which was filled with linseed oil, thoroughly stirred, and of uniform temperature throughout.

Temperature.	Resistance. The Res. at 0° C. being = 1.
0°	1·0
10	1·0024
20	1·0044
30	1·0075
40	1·0066
50	1·0097
60	1·0126
70	1·0134
80	1·0166
90	1·0188
100	1·0209

This gives for the average percentage variation of resistance per 1° C., between the temperatures 0° C. and 100° C., the number

0·02087. A second wire tested very carefully in a similar way gave for this average percentage variation between 0° and 100°, 0·022 per degree, with a steadily increasing rate of variation from the beginning.

To compare this increase in resistance due to increase of temperature with that observed in other metals and alloys, I find that the percentage increase of resistance for 1° C. at 20° C. for copper is 0·388, platinum-silver alloy 0·031, gold-silver alloy 0·065, and for German silver 0·044. These numbers were obtained by Matthiessen in the course of his experiments for finding a suitable metal or alloy for the purpose of constructing the British Association standards of electric resistance. It appears that the variation of resistance of platinoid with temperature is very much smaller than the smallest observed for any of the metals and alloys then examined.

Addition to the above Paper. Received May 12, 1885.

I have now (May 7th, 1885) obtained some information as to the mechanical properties of platinoid wire.

I have determined the modulus of rigidity of a wire of the substance, a portion of the wire marked A in the preceding paper being used for the purpose. This wire is a little larger than No. 24 wire of the Board of Trade Standard wire gauge, and has a diameter of 0·0595 cm. To determine the modulus of rigidity a cylindrical ring vibrator was attached by a cross-bar to the lower end of a length of the wire, the upper end being fixed by soldering to a brass plate, which was screwed to a beam in the roof of the laboratory. The moment of inertia of the cylindrical vibrator and cross-bar was ascertained; and the torsional vibrations of the vibrator hung by the wire were counted, and the period determined.* From this, and the dimensions of the wire, the modulus of rigidity is calculated.

The following are the particulars of the experiment:—

Length of wire used (l)	490·8 cm.
Diameter of wire ($2a$)	0·0595 „
Moment of inertia of vibrator (Mk^2)	29453·2
Time of vibration one way (half period) (T)	16·375 secs.
Rigidity in grammes weight per square cm. $\left(\frac{2\pi l M k^2}{g T^2 a^4}\right)$	$475·8 \times 10^6$.

The Young's Modulus, or modulus for elastic longitudinal extension, and the breaking weight, have been determined for me by Mr. Magnus Maclean, Official Assistant to the Professor of Natural-

* Sir William Thomson, "Elasticity and Viscosity of Metals," "Proc. Roy. Soc.," 1865.

Philosophy in the University of Glasgow, who also assisted me in the detemination of the rigidity modulus; they are as follows:—

Young's Modulus, ($Pl/\alpha\epsilon$ where P is the stretching weight, l the length of wire used, α the cross sectional area, and ϵ the elongation produced by the pull P) $1222 \cdot 4 \times 10^6$ grammes weight per square centimetre.

The breaking weight is about $6 \cdot 029 \times 10^6$ grammes per square centimetre.

I have also determined the specific gravity of a specimen of platinoid wire, and find it to be 8·78, compared with water at 20° C.

Platinoid when drawn hard, is softened like copper by heating and sudden cooling.

V. "Results of the Harmonic Analysis of Tidal Observations." By Major A. W. BAIRD, R.E., and G. H. DARWIN, F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received March 19, 1885.

[Publication deferred.]

The Society adjourned over Ascension Day to Thursday, May 21st.

May 21, 1885.

Mr. WARREN DE LA RUE, Vice-President, in the Chair.

The Right Hon. Lord Justice Sir Charles Synge C. Bowen, whose certificate had been suspended, as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Contributions to the History of the Pleiocene and Pleistocene Deer. Part I. *Cervus verticornis*, *Cervus Savini*." By W. BOYD DAWKINS, M.A., F.R.S., F.G.S., Professor of Geology and Palæontology in the Victoria University. Received April 27, 1885.

(Abstract.)

The numerous cervine remains which occur in the various collections in Britain and on the Continent have been studied by the author for the last twenty-five years, and in this communication two species, the one hitherto ill-defined, and the other new to science, have been described.

The first, or *Cervus verticornis*, Dawkins, remarkable for the singular forward and downward curvature of the first tine, is represented by a large series of skulls and antlers, which enable the author to define the changes in antler-form from youth to old age, as well as to relegate it to the division of deer with palmated antlers, and to establish its geological age to be Pleiocene, and early Pleistocene, in Norfolk and Suffolk.

The second, or *Cervus Savini*, is represented by several skulls and many antlers, which present considerable modifications in form at varying ages. It also belongs to the section of deer with palmated antlers, and is probably the ancestral form of the extinct (*Cervus Browni*, Dawkins) and living (*C. dama*) types of fallow deer. It has hitherto only been met with in the early Pleistocene forest-bed series of Norfolk and Suffolk.

II. "On Beds of Sponge-remains in the Lower and Upper Greensand of the South of England." By GEORGE JENNINGS HINDE, Ph.D., F.G.S. Communicated by HENRY WOODWARD, LL.D., F.R.S. Received April 29, 1885. Read May 21.

(Abstract.)

I have pointed out in this paper the occurrence in the Lower and Upper Greensand strata of the Wealden area, the Isle of Wight and the south-western counties, of beds of rock formed to a large extent of the detached spicular remains of siliceous sponges, and thus distinctly of organic origin. Their true characters have not been generally recognised, and they have usually been described as deposits of sandstone, chert, malm, hearthstone, firestone, &c. In the Lower Greensand these beds are mainly developed in the lower, or Hythe, division, and they are exposed at Haslemere, Midhurst, Petworth, Godalming, Tilburstow Hill near Godstone, Sevenoaks, Maidstone, and at Hythe. The sponge-beds vary from three-quarters of an inch to three feet in thickness; between them, as a rule, there are intervening beds of sand or sandstone. The greatest total thickness of the sponge-beds exposed in any one section is 11 feet. Sponge-beds are less common in the higher or Folkestone division of the Lower Greensand, but they are numerous at Folkestone itself, and reach a total thickness of more than 8 feet, and there is also a thin bed in this division at Sevenoaks. The Lower Greensand strata at Faringdon, in Berkshire, are of an altogether different character to those of the same formation in the area treated of in this paper, and the sponges which abound therein are likewise entirely different, being calci sponges, and retaining their entire forms.

The sponge-beds in the Upper Greensand are of two distinct types, one of which is shown on the northern and western margin of the Weald, and the other in the Isle of Wight, and further westward in the counties of Wilts, Somerset, Dorset, and Devon. In the first-named district the sponge-beds are of a soft, greyish-white, siliceous, or siliceo-calcareous rock, known under the names of malm, hearth, or firestone. In this the sponge spicules principally occur in the negative form of minute empty casts, the presence of which renders the rock extremely light and porous. The beds can be traced nearly continuously along the northern and western margin of the Wealden area, and they are well shown at Godstone, Merstham, near Reigate, Betchworth, Farnham, and Selborne. Further northwards they are present at Wallingford, in Berkshire. The beds vary in thickness from 15 feet at Merstham to 60 feet at Farnham.

In the more typical Upper Greensand of the Isle of Wight and the

south-western counties, the sponge-beds consist of thick layers of chert and porous siliceous rock at the summit of the series, immediately beneath the so-called chloritic marl; whilst in the lower division the sponge-remains principally occur in loose quartzitic sands with siliceous accretions. The chert or sponge-beds at the top of the Upper Greensand are best exposed at Shanklin, Ventnor, and the Undercliff, in the Isle of Wight, at Warminster in Wiltshire, and Penzlewood in Somersetshire. They vary from 10 to 25 feet in thickness. The sponge-beds of the lower division are scarcely recognisable in the Isle of Wight, but they attain a thickness of 10 to 20 feet on the summit of the Blackdown and Haldon Hills in Devonshire, and at Axmouth in Dorsetshire. The chert here is only present in beds of subordinate importance.

Sponge-beds of similar characters to those of the Greensand have been described from the Hilssandstein in Westphalia, which is of Neocomian age, and, judging from specimens which I have examined, the "Gaize de l'Argonne," which is largely developed in the Ardennes, and the "Meule de Bracquegnies" in Belgium, are sponge-beds, filled with spicules and spicular casts like those of the Greensand.

The sponge-remains in the various beds are exclusively those of siliceous sponges. In some the silica of the spicules yet retains its original colloidal condition, in which it is negative to polarised light and readily soluble in caustic potash. The matrix of the sponge-beds of the malm and firestone is also to a large extent of colloidal or amorphous silica, and this material has been deposited in the form of minute globules or disks, and seems to have been derived from the sponge spicules, with the empty casts of which the beds are throughout filled.

More generally the original amorphous silica of the sponge-remains has been altered to chalcedony, and the chert and porous siliceous rock accompanying it, which is filled with traces of the spicules, are likewise of chalcedony; occasionally the chalcedony gives place to crystalline silica.

Glauconite very commonly fills the canals of the spicules, and remains after the spicular walls have been removed; it also replaces the spicular walls.

In some sponge-beds the spicules have been nearly entirely replaced by crystalline calcite; they are embedded in a matrix of granular limestone.

As a general rule the sponge-spicules are inclosed in a compact matrix in which their forms can only be partially studied, but under certain conditions they are loosely distributed in sand, or in fine powder in cavities in chert, from whence they can be obtained quite free from matrix. The sponge-beds appear to be composed of detached, free, spicules of disintegrated sponges; entire sponges are absent.

These spicules belong to numerous species. All four orders of siliceous sponges are represented, but whilst the Monactinellid and Hexactinellid sponges form but a small proportion, the Tetractinellid and Lithistid sponges, more particularly those of the Megamorina family, are extremely abundant.

III. "The Solar Spectrum from λ7150 to λ10,000." By Capt. ABNEY, R.E., F.R.S. Received May 6, 1885.

(Abstract.)

The paper deals with the method employed in taking the photographs of the solar spectrum, from which the map accompanying it was made, and indicates the degree of accuracy which has been obtained.

IV. "On Charging Secondary Batteries." By WILLIAM HENRY PREECE, F.R.S. Received May 6, 1885.

1. I have for the past twelve months been experimenting with secondary batteries with a view of getting an efficient, uniform, and constant source of currents for electric lighting my house, and I have succeeded beyond my expectations. Some new facts have developed themselves during my experience, which I have thought of sufficient importance to bring before the Society.

2. The cells are of the Planté type, manufactured by the Elwell Parker Company of Wolverhampton. Fourteen plates of plain sheet lead 17″ × 11″ are suspended in well insulated wood boxes filled with diluted sulphuric acid in the proportion of about 1 to 10. These plates are grouped in two groups of seven, each group being soldered to a lead strip, forming alternately the positive and negative poles of the cell. The plates of the respective poles are prevented from touching each other by ebonite grids or separators.* Each plate offers a surface of 1·3 square feet, so that the total surface of lead of each group opposed to each other is 9·1 square feet; that is, 9·1 square feet of peroxidised lead is opposed to 9·1 square feet of spongy lead. I use twenty-four such cells.

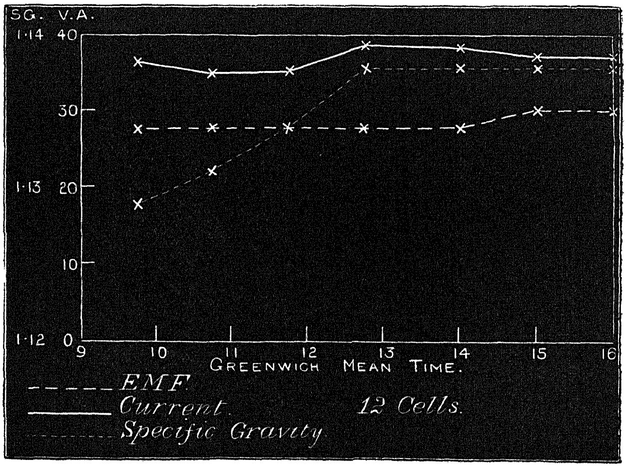
3. My charging current varies from 3 to 3½ ampères per square foot, while the current of discharge used in lighting my house varies from 1 to 1½ ampère per square foot. The total weight of each cell is

* These ebonite grids were introduced by Mr. Charles Moseley, and have effectually removed one incessant source of trouble in these cells, viz., short-circuiting through the buckling of the plates.

120 lbs. The plates are prepared by the Parker-Planté process before insertion in the cell, those forming the positive pole being well peroxidised, while those forming the negative pole are well coated with spongy lead.* They are thus, when put together, prepared at once to be charged. If they are not at once charged, local action sets in, and lead sulphate is injuriously formed.

4. I have cut away a small portion of the centre of the central plate of each cell to admit a hydrometer having a scale graduated from 1·050 to 1·150. The changes of the density of the liquid and of the colour of the plates give the fullest and clearest indications of the behaviour of the cell. The condition of the surface of the peroxidised plate as felt by the finger is also good evidence of its condition. If a plate is yellowish-brown and rough, it probably makes bad contact with the lead terminal; if black and hard, it wants the density of current regulated; if plum-coloured and greasy to the touch, it is in good order, and working well.

5. At the present time I am charging my battery twice a week, putting in at each charge about 120 ampère-hours. The battery is charged in two sections. During each charge observations are made

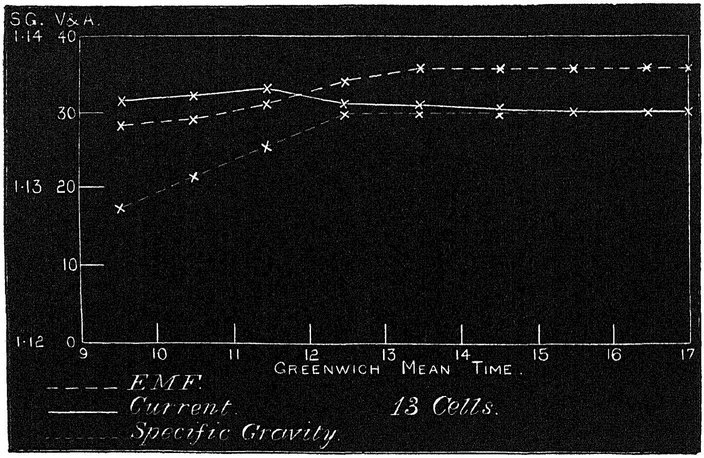


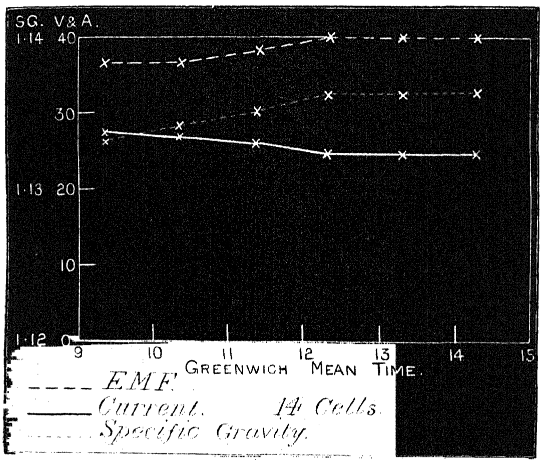
* The Parker-Planté process consists in immersing for a few hours the lead plates in a solution of nitric and sulphuric acids in these proportions—

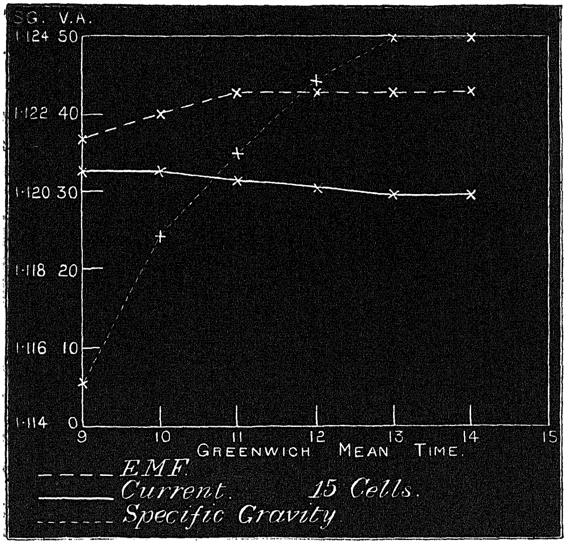
Nitric acid	1
Sulphuric acid	2
Water	17

before fixing in the cells. This not only chemically cleans the lead surfaces, but it favours the formation of sulphate of lead in such a way as to be readily converted into lead peroxide and spongy lead on the passage of a strong current through the cells. The formation of the cells is thus greatly expedited.

every hour, and sometimes every half-hour, (1) of the strength of current flowing through; (2) of the electromotive force; and (3) of the density of the liquid of one or more of the cells. The relation of these three quantities tells me the condition of the charge. The accompanying diagrams are typical cases.







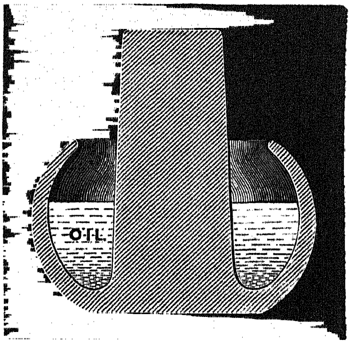
Take No. 4, which is the diagram of this morning's (April 28) charge of 15 cells. The following are the particulars:—

	E.	C.	D.
9.0 A.M.	37·7	32·24	1·115
10.0 „	40·2	32·24	1·119
11.0 „	42·5	31·50	1·121
12.0 noon	42·5	30·74	1·123
1.0 P.M.	42·5	29·95	1·124
2.0 „	42·5	29·95	1·124

6. It is evident that after four hours' continuous charging the battery was full, for the density, the electromotive force, and the strength of current became constant. The same scale of ordinates is used for volts and ampères. When each magnitude reaches its constant bubbles of gas are freely given forth, and energy is being wasted. The variation of the electromotive force and current strength is clearly due to the counter-electromotive force of the cells, which becomes a maximum only when the plates are fully formed. The counter-electromotive force partakes of the character of a higher resistance opposing the charging current, and increasing the proportion of the current through the shunt of the dynamo. Hence the changes of electromotive force are more marked than those of the current. Indeed, the changes in the electromotive force, as given by the voltmeter, are sufficient alone to indicate the progress and completion of the charge. They are more reliable than the evolution of gas. My dynamo is a shunt-wound Gramme, my voltmeter is Cardew's,

a most reliable, simple, and valuable instrument, and my current meter a Siemens' electrodynamometer.

7. I was at first much troubled with electrical leakage. The current escaped over the edges of the box through creeping by capillary action, and the formation of moisture from the spray of the solution when bubbles of gas arose in the liquid and burst on the surface. This was quite cured by standing each box on three white porcelain supports of the form shown in the following figure, the



cups being half filled with resin oil on Messrs. Johnson and Phillips' plan. It is now quite impossible to measure the leakage except with a delicate galvanometer, and the insulation may be said to be practically perfect.

8. The E.M.F. of the battery at its terminals:—

When charging	2·25 per cell.
When idle	2·05 ,,
When discharging	1·90 ,,

The internal resistance* is, per cell:—

When charging	·0060$^{\omega}$
When discharging	·0017$^{\omega}$.

9. But the latter varies very markedly with the strength of current of discharge. This is shown by the following experiment made with 23 cells of a smaller type than those described above, which are used in the Post Office.

Current of discharge in ampères.	Internal resistance in ohms.
4·39	0·7608
7·25	0·4607
15·84	0·2816
25·07	0·1969

* The term "internal resistance" means the effect of counter-electromotive force as well as of resistance to conduction.

10. Thinking that this remarkable diminution of internal resistance might be due to the evolution of heat, I measured the temperature with a delicate thermometer.

Normal temperature of cell $12\frac{1}{2}°$ C. Current of discharge:—

5 ampères	No alteration of temperature perceived.
10 „	An exceedingly slight change.
16 „	About $12\frac{3}{4}°$.
20 „	Barely 13°.

The current in each case was kept on for 20 minutes, hence the diminution is not due to heat. Since the internal resistance varies in this way, I now always take the internal resistance with the same current, viz., 10 ampères.

11. The capacity of these batteries certainly improves with age, and up to the present time I have seen no sign of decay or deterioration. M. Planté informed me that, though in course of time the peroxidised plate becomes very brittle, it is impossible to peroxidise it completely through; there always remains a metallic core to give it strength. My experience of these plates confirms this. Up to the present moment I have made no careful measurements of the efficiency of my battery. I cannot do so without deranging the lighting of my house, and I regard observations on single cells as illusory. I put in about 240, and I take out about 200 ampère-hours weekly, and I do not observe any change or fall in the electromotive force. When the electromotive force of these cells falls, it falls rapidly, indeed, almost suddenly. Occasionally one plate of a group becomes inactive from undue local action, or from bad connexion (shown by the colour). I remove this plate and put it in what I call a "hospital" cell, where it is brought into order either by a greater density of current or by reversal. Now that my plates have been in use for some time they seem far less inclined to local action. In the oldest cells there is no trace of local action.

12. Reversing has a great beneficial action on a cell; it not only improves its capacity, but it removes any cause of irregular working. It is advisable to do this periodically. I have two extra cells, which enables me to have two cells always under reversal by means of the charging current. It takes from 1,000 to 1,200 ampère-hours to reverse a cell in its present condition, so that at this time of year it takes a month or more to complete the operation, and it will take a year to reverse the whole battery. I have thus, up to the present time, reversed 16 cells.

V. "The Development of *Peripatus Capensis*." By ADAM SEDGWICK, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received May 9, 1885.

The development of *Peripatus capensis* was first studied by Moseley, who stopped for a short time at the Cape in November and December some years ago. His observations related only to a few stages comparatively late in development. Balfour, in 1882, found some younger embryos in specimens collected by Mr. Lloyd Morgan in July and August. He had only time to make a very few observations, of which he left a short record in the form of four rough drawings and a short note, and a letter to Prof. Kleinenberg, before starting on his last expedition to Switzerland. His observations were so interesting that they were made the subject of a short communication to the Royal Society in the autumn of 1882, and they were slightly extended by the editors of his last work on the anatomy of *Peripatus capensis*, and published with that monograph in the "Quarterly Journal of Microscopical Science" in the spring of 1883.

The subject seemed so important that the Government Grant Committee of the Royal Society granted, in the spring of 1883, the sum of £100 to enable me to go to the Cape for the purpose of obtaining well-preserved embryos, and of studying the development on fresh specimens.

Accordingly, I went to the Cape in the summer of 1883, arriving early in July, and remaining till the middle of August. I obtained a large number of specimens, and brought back with me over 300 alive. Some of the latter lived at Cambridge till the following July. The results of my observations at the Cape and after my return to England have been to show that the embryos remain thirteen months in the uterus; that the fertilised ova pass into the uterus in April, and the young are born, fully developed, in the May of the year following. That is to say, the young ova pass into the uterus one month before last year's young are born. I was not prepared for this, and I did not, in 1884, examine my specimens for the early stages until May, when the young were being born. The result was that I missed last year the early stages of development, and had it not been for the kindness of Mr. Walter Heape, who went to South Africa last summer, and who collected and brought back some more live specimens, I should have been obliged to leave the early stages undescribed. Thanks to him, however, and to my experience gained last year, I have this April been able to find several of the younger stages, and to complete my observations.

The testes are fully developed and charged with ripe spermatozoa

in our autumn, when the males deposit spermatophors quite casually all over the body of the female, and continue to do so until about January (perhaps a little later).

The ovaries always contain spermatozoa, but in smaller numbers, directly after the eggs have passed into the oviduct, than at any other time.

How the spermatozoa pass up the uterus and oviducts, which are always full of embryos, generally containing as many as thirty to forty, I do not know.

Fertilisation is apparently effected in the ovary. The fertilised ova pass into the oviduct in April, while the uterus is still full of embryos almost ready for birth. Segmentation and the early stages of development take place in the oviduct, where the eggs remain until May, when the young are born. They then pass into the uterus to remain there until the following May.

The ripe ovum is elliptical in shape, and of a dark colour by transmitted light. The opacity is due to the presence of granules (food yolk). It is enclosed, when in the oviduct and uterus, by a transparent structureless membrane, which is derived apparently from the follicular cells surrounding the ovarian ovum.

Segmentation is complete. The first furrow is in the transverse plane of the ovum and divides it into two halves. The second furrow is at right angles to the first, and divides each of the first formed segments into two. Then each segment divides into two, one small and one large segment. There are thus four small segments lying close together on one side of the egg and four large segments. The former give rise to the ectoderm, and the latter to the endoderm. The subsequent division of these two kinds of cells proceeds independently. At the end of the segmentation, the ovum consists of a number of large endoderm cells *scattered irregularly* within the egg membrane, while the ectoderm cells form a mosaic of cells closely applied together and placed close to the membrane on one side at about the middle of the long axis of the egg.

The endoderm cells now draw together and form a solid mass of cells on which the ectoderm rests like a cap.

The ectoderm then grows round the endoderm cells, and completely encloses them, excepting at one point (opposite to that on which the ectoderm cap was placed).

The embryo thus acquires a spherical form, and consists of a solid gastrula, the small uncovered spot of endoderm constituting the blastopore.

A cavity then appears in the centre of the endoderm cells, and this cavity opens to the exterior through the blastopore.

The embryo now becomes slightly elliptical, and the blastopore also elongates in the direction of the long axis.

An opacity appears at the hind end of the blastopore. This opacity is the primitive streak. It appears to be due to the active proliferation of some cells, which cannot be definitely assigned either to the ectoderm or the endoderm, at the hind end of the blastopore. This stage, which has already been described as stage A in Balfour's Memoir on Peripatus ("Quarterly Journal of Microscopical Science," vol. 23, Plate 20, fig. 34), is found most commonly at about the middle of June.

The embryo now grows considerably in length, the blastopore presenting a corresponding elongation, and the mesoderm, which arises from the proliferation of the undifferentiated cells of the primitive streak, grows forward as two ventro-lateral bands, one on each side of the blastopore.

The mesodermal bands next divide by transverse division from before backwards into somites, which contain a cavity, part of the future body cavity. The first somite to appear is the anterior, and then successively backwards.

The blastopore now divides into two parts by the obliteration of its median portion into an anterior part which becomes the mouth of the adult, and a posterior part which is at first placed at some little distance from the hind end of the embryo and gives rise to the anus of the adult.*

The primitive streak still persists and extends from the hind end of the blastopore to the hind end of the embryo. It is now marked by a groove—the primitive groove.

The anterior pair of somites have shifted forward to quite the anterior end of the body; they give rise to the mesoderm and body cavity of the præoral lobes.

This stage has already been described and figured as stage D in Balfour's memoir (*op. cit.*, Plate 20, fig. 37).

The hind end of the body now becomes curved ventrally. The beginning of this curve is shown in the figure of stage E in Balfour's memoir (Plate 20, fig. 38). The curve is produced by the growth of the hind end of the body. As this growth proceeds the curve becomes more marked, and assumes a spiral form, that is to say, the hind end of the body is spirally coiled, the coil being applied to the ventral face of the anterior part of the body. (*Vide* Moseley's figure in the "Phil. Trans.," vol. 164, Plate 75, fig. 1.)

* There is no structure in the embryos of *Peripatus capensis* corresponding to the amnion or placenta described by v. Kennel in the West Indian species (Semper's "Arbeiten," Bd. 7). The early stages of that species are obviously difficult to follow, and have not been entirely made out by that observer. I think it, therefore, better not to offer any criticisms on his statements, particularly those with regard to the amnion, until his observations have been either extended or confirmed.—(May 25, 1885.)

The spiral soon straightens out, and the embryo becomes bent double, the ventral surface of the hind part of the body being applied to the ventral surface of the front portion, the tail end of the embryo being curled round the front end of the head. The bend occurs at the level of the 8th somite. This stage has been figured by Moseley ("Phil Trans.," vol. 164, Plate 75, fig. 4).

The embryo gains this form at the beginning of October, and retains it until birth. The most important internal changes take place between the stage called E and this stage.

The spiral stage is characterised by the presence of the full number of somites, the disappearance of the primitive streak, and the shifting of the anus to the hind end of the body. The appendages also begin to appear in this stage. They arise as hollow processes of the body wall, containing prolongations of the somites. The first pair to appear are the antennæ, into which the præoral somites are prolonged. The remainder appear from before backwards in regular order, viz., jaws, oral papillæ, legs, 1, 2, . . . 17, and the rudimentary anal papillæ which are the appendages of the last, *i.e.*, the 21st somite. The end of the spiral stage is also characterised by the appearance of the buccal fold or fold which encloses the jaws and buccal cavity, and so constitutes the tumid lips of the adult. This is a fold of the side walls of the body immediately outside the jaws, and extending from the præoral lobe of its side to just behind the jaw. This is most marked in front, which fact led Moseley to describe it as a backward process of the præoral lobe.

I now pass to the internal changes, by which the various organs are established.

THE ECTODERM.

The ectoderm, excepting at the points where it gives rise to the nervous system, is always one-layered. During stage E the cells on the median portions of the dorsal and ventral surfaces are extremely flat; those on the sides are columnar, and especially so over the ventral parts of the sides of the body. Here indeed the ectoderm becomes during the spiral stage more than one cell thick, and gives rise to a thickening extending along the whole length of the body, passing forward on each side of the mouth on to the præoral lobes.

Nervous System.—The entire central nervous system develops from these continuous ventro-lateral thickenings of the ectoderm. The ventral nerve cords and œsophageal commissures arise from the deeper rounded elements of the thickenings as two ventro-lateral cords, which in front of the mouth are still continuous with the ectoderm.

The nervous ectodermal thickenings in front of the mouth are enormously developed, even during the spiral stage, and the nervous

rudiments in this region remain connected with the surface ectoderm much longer than do those of the hinder parts of the body. In fact it may be said that the præoral part of the central nervous system *never does separate from the ectoderm which gives it origin, inasmuch as the latter is invaginated in the form of two longitudinal furrows*, which soon become deeper, and eventually closed, in exactly the same manner that the medullary groove of a vertebrate embryo is closed. Two closed vesicles thus originate, one for each of the future cerebral ganglia. These vesicles are lined by ectoderm, and by that particular ectoderm from which the nerve elements of the ganglia originate: *they persist throughout life as the small hollow appendages of the supra-œsophageal ganglia*, described and figured by Balfour in his memoir (*op. cit.*, Plate 17, figs. C, D).

The **eyes** arise as invaginations of the sides of the nervous thickenings of the præoral lobes during the spiral stage. The invaginations are at first shallow, but soon become deeper, and eventually converted into closed vessels, the front wall of which (*i.e.*, the wall next the skin) forms the epithelium outside the so-called lens of the adult eye, while the internal wall thickens, and remains continuous with the cerebral ganglion, and gives rise to the retina. The enclosed vesicle persists and apparently becomes filled by the structureless lens of the adult eye, if the structure described as such be not a mere coagulum produced by reagents. The eye of Peripatus is therefore a cerebral eye.

Stomodæum and Proctodæum.—The ectoderm grows inwards for a short distance, at the front and hind ends of the alimentary canal. The anterior ingrowth gives rise to the lining of the pharynx, and possibly—but of this I am not yet certain—the œsophagus of the adult. The buccal cavity of the adult is formed, as I have already pointed out, by a fold of the integument which encloses the jaw and external openings of the salivary gland. The posterior ingrowth forms the lining of the rectum.

The Endoderm.

The endoderm arises from the large segmentation cells which arrange themselves round a cavity, the future mesenteron.

The mesenteron, from its very first appearance, opens to the exterior. *This opening or blastopore gives rise, as described above, to the permanent front and hind openings of the alimentary canal.*

The mesenteron possesses no glandular appendages of any kind whatsoever.

The Mesoderm.

The mesoderm arises, as described above, from the undifferentiated cells of the primitive streak of the early stages. It is completely

established by the spiral stage, when the primitive streak vanishes, and the anus shifts to the hind end of the body. In the original account of the early stages in Balfour's memoir, it was stated by the editors (p. 256) that the greater portion of the mesoderm was formed from the primitive streak.

I am now in a position to state that the whole of the mesoderm originates in this manner.

On the view that the primitive streak is the remains of a portion of the blastopore and the ingrowth of cells from it a modified development of that part of the ancestral archenteron which gave rise in the ancestor to the somites, the mesoderm of Peripatus is to be regarded as developing from the walls of the archenteron.*

The mesoblastic bands at once become segmented, a cavity appearing in each somite as it becomes marked off.

The anterior somite, which is the first to be formed, is at first placed some little distance behind the front end of the blastopore. It soon shifts forward, and by stage E is placed in front of the embryonic mouth.

The somites are from the first placed at the sides of the mesenteron. When the appendages are formed they send prolongations into them; this remark applies to the antennæ as well as to the other appendages.

The **body cavity** of the adult is very complicated, and divided into several parts, viz.: (1) the main central division containing the gut; (2) two lateral chambers separated from (1) by a longitudinal vertical septum, and containing the nerve cords; (3) the body cavities of the legs, separated from (2) by a vertical septum, and containing the segmental organs and crural glands; (4) the pericardial cavity. Of these (1) and (2) are derived, not from the cavity of the somites, but from a space which is formed by the separation of the endoderm from the ectoderm, and from the inner wall of the somites, these being at first in contact. At first these divisions of the body cavity have no special lining of cells, but subsequently migratory cells from the somites apply themselves to the endoderm and ectoderm, and form the very sparse lining found in the adult.

The cavities in the legs are derived from the cavities of the somites, the changes of which I will now shortly relate, so far as I have been able to follow them. I regret to say that there are one or two points,

* It was from this point of view that the editors of Balfour's memoir (*op. cit.*) made the second statement at the bottom of p. 256 with regard to the mesoderm. This statement, which runs thus, " The greater part of the mesoblast is developed from the walls of the archenteron," is obviously at variance with the statement already referred to at the top of the same page, unless considered in the light of the above hypothesis. It was meant to be so considered, and I regret that the omission of the explanation now given has led to some misunderstanding.

indicated below, with regard to these changes which, are still somewhat obscure.

When the appendages appear, the somites become prolonged into them. The portion of the somite in the leg soon (at about the end of the spiral stage) becomes separated from the remainder, which shifts dorsalwards, and comes into contact with its fellow along the dorsal middle line above the gut. I will in future refer to the former as the leg division of the somites, and to the latter as the dorsal division.

The leg division becomes inconspicuous in the antennæ and jaws. In the remaining appendages (oral papillæ and seventeen pairs of legs) it acquires an opening to the exterior at the base of each appendage and immediately outside the nerve cords.

The portion of the leg body cavity, which thus communicates with the exterior, gives rise to the **segmental organs**. The segmental organs are never completely separated from the rest of the leg body cavity, except in the case of the first, the aperture of communication persisting as the internal opening of the nephridium.

The anterior segmental organ, viz., that of the oral papilla, gives rise to the salivary glands of the adult.*

The segmental organs then develop in Peripatus, as they do in Elasmobranchs, by the *direct modification of parts of the primitive body cavity*. These constitute, I believe, the only two cases in which such a method of development of these organs has been observed.

The cells of the splanchnic lining of these dorsal divisions of the body cavity contain during the spiral stage the primitive **generative** cells. But it is difficult to say where the generative cells originate, because during the spiral and later stages nuclei exactly similar to those of these cells are placed in groups in the portion of the endoderm which is in contact with that part of the splanchnic mesoderm which contains the generative cells. I cannot therefore be certain that the generative cells do not arise in the endoderm and subsequently migrate into the splanchnic mesoderm. However that may be, they soon become surrounded by some of the smaller flat cells of the mesoderm which constitute the egg follicle. So far the development of the somites is quite clear and easy to follow. But the changes by which the dorsal divisions of the somites are converted into their permanent form take place at a late period of development —during November—and are, in consequence of the thinness of the walls, extremely difficult to follow. I have not succeeded entirely in following them.

I will content myself, therefore, with making the following statement, of the truth of which I am by no means confident.

The dorsal divisions unite with each other transversely and longi-

* The opening marked *d* in fig. 8, Plate 75, of Moseley's paper (*op. cit.*) is the opening of the salivary gland, not of the slime gland as he states (see below).

tudinally, and give rise to a continuous cavity—the pericardial cavity. The portion of this cavity containing the generative cells become separated from the rest as two tubes which form the generative glands and part of their ducts, and come to lie ventral to the pericardium in the central compartment of the body cavity. The external parts of the generative ducts appear to be derived from the modified leg cavity of the anal papillæ.

I have no satisfactory observations on the development of the **heart**; but it appears to arise from the cells in the dorsal middle line which intervene between the two dorsal divisions of the somites before they unite with one another across the middle line.

The **slime glands** are epiblastic in origin. They first appear as an invagination of ectoderm, at the apex of each oral papillæ. They have nothing to do with nephridia.

I have no observations on the development of the **crural glands**, neither have I been able to see anything of the development of tracheæ. I trust that the Society will deem the foregoing results of sufficient interest to justify this short preliminary, but I fear too long delayed, account of my observations on the development of *Peripatus capensis*. The delay has been occasioned by the fulfilment of engagements which were undertaken before I went to the Cape, and have proved more arduous than I had anticipated. My observations are nearly completed, and the greater part of the drawings are made; and I trust that the detailed account will soon—before the autumn—be ready for publication.

The Society adjourned over the Whitsuntide Recess to Thursday, June 11.

Presents, April 16, 1885.

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Portrait Seal of Sir Isaac Newton. Presented by Mr. F. C. Bayard.

June 4, 1885.

The Annual Meeting for the Election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, General Boileau and Sir Erasmus Ommanney were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society:—

Baird, A. W., Major, R.E.
Carpenter, Philip Herbert, D.Sc.
Clark, Sir Andrew, Bart., M.D.
Common, Andrew Ainslie, F.R.A.S.
Creak, Ettrick William, Staff Commander, R.N.
Divers, Prof. Edward.
Hicks, Henry, M.D.
Hicks, Prof. William Mitchinson, M.A.
Japp, Francis R., Ph.D.
Marshall, Prof. Arthur Milnes, M.D.
Martin, Prof. Henry Newell, D.Sc.
O'Sullivan, Cornelius.
Perry, Prof. John.
Ringer, Prof. Sydney.
Vines, Sidney Howard, D.Sc.

Thanks were given to the Scrutators.

June 11, 1885.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Lord Justice Sir Charles Synge C. Bowen, Dr. Philip Herbert Carpenter, Mr. Andrew Ainslie Common, Staff-Commander Ettrick William Creak, Dr. Henry Hicks, Professor John Perry, and Professor Sydney Ringer were admitted into the Society.

THE BAKERIAN LECTURE—"On the Corona of the Sun"—was delivered by WILLIAM HUGGINS, D.C.L., LL.D., F.R.S.

[Publication deferred.]

June 18, 1885.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Sir Andrew Clark, Bart., Dr. Francis R. Japp, Dr. Arthur Milnes Marshall, and Mr. William Mitchinson Hicks were admitted into the Society.

The following Papers were read:—

I. "The Action of Tidal Streams on Metals during Diffusion of Salt and Fresh Water. Experimental Research. Part II (Gravimetric)." By THOMAS ANDREWS, F.R.S.E., Wortley Iron Works, near Sheffield. Communicated by Professor G. G. STOKES, Sec. R.S. Received May 29, 1885.

In a paper last session "On the Electromotive Force during Diffusion in Tidal Streams" (see "Proceedings Royal Society," No. 232), the author recorded the electrical part of this investigation. The present communication contains the concluding gravimetric experiments of the research. The effects attending the diffusion of the salt and fresh water in tidal estuaries, on parts of the same metal, were estimated in each case for a period of *one year*, during which the metals were constantly exposed to conditions of galvanic action similar to those obtaining in some tidal streams. The results demonstrate that electric disintegration of the nature alluded to in this and the former paper (viz., the galvanic destructive action on parts of even the same metal, arising from difference of potential during diffusion between the surface and lower waters in a tidal stream) is, on comparison with other investigations by the author, apparently of much greater extent than the loss either from simple corrosion in sea water alone, or than that which ensues from the action on each other of dissimilar metals of this group (such as wrought iron, cast metals and steels) in galvanic connexion in sea water. The wrought iron, steels, and cast metals used were from the author's standard samples of known composition in the form of

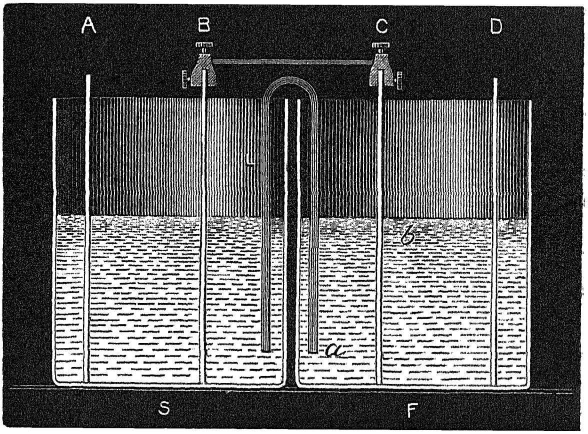
bright polished plates, of similar manufacture and general composition to the metals employed in the former paper.

Table A.—Analysis of the Wrought Iron, Steels, and Cast Metals employed.

Description.	Graphitic carbon.	Combined carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Iron (by difference).	Total.
Wrought iron	...	none	0·392	0·034	0·270	0·194	99·110	100·000
"Soft" Bessemer steel	...	0·150	0·015	0·111	0·064	0·540	99·120	100·000
"Soft" Siemens-Martin steel	...	0·170	0·071	0·117	0·077	0·627	98·938	100·000
"Soft" cast steel	...	0·460	0·074	0·025	0·210	0·184	99·047	100·000
"Hard" Bessemer steel	...	0·510	0·068	0·113	0·087	1·153	98·069	100·000
Best cast metal, No. 1	2·780	0·390	2·340	0·090	0·580	0·450	93·370	100·000
Common cast metal, No. 2	2·620	0·670	1·940	0·090	0·950	0·520	93·210	100·000

The exposed part of each plate was 3 inches square by $\frac{1}{8}$ inch thick. There was, however, a shank (3 inches long by $\frac{1}{8}$ inch square) left in the centre of each plate for connecting purposes. The method of experimentation was as follows, see sketch, fig. 1: various interesting results were obtained by this arrangement of the cells.

FIG. 1.



Sketch of one set of the Diffusion Cells.

Four plates, A, B, C, and D (cut from one large plate of each of the metals, so that the four experimental plates of each set were practically of the same composition, molecular structure, &c.), were arranged as shown in fig. 1, each plate having been previously dried at 212° F., and weighed on the balance.

The plate A standing alone and unconnected was immersed in the vessel S containing a measured quantity of sea water.

The plate D also stood unconnected in the vessel F, and the action on this plate was similar to that on plate C, though somewhat modified in extent. This plate, although standing isolated, afforded another confirmatory indication of the extensive corrosion of metals exposed in water less saline at the surface than near the bottom, notwithstanding that the total salinity of the water in vessel F was greatly below that of the sea water.

The plate B was immersed in the sea water, but galvanically connected with the plate C in the vessel F.

In the case of the last two plates B and C, circuit was completed, and slow but continuous diffusion between the two solutions accomplished by means of a triple fold of soft moistened chamois-leather L (1 inch wide), one end of which was immersed deeply in the sea water, the other to an equal depth in the distilled water, see fig. 1. The sea water was obtained regularly from Filey Bay. The vessel was filled at the commencement with 25 fluid ounces of sea water, the vessel F with the same volume of distilled water. Both were emptied by siphon (without disturbing the plates) and each carefully refilled with their respective waters periodically every fourteen days for a total period of *fifty-two weeks.* Seven pairs of such diffusion cells of equal size were employed in the investigation. At the termination the plates were taken out, washed, carefully cleaned, dried at 212° F., and again weighed on the balance, and the corrosive effects determined by their relative loss in weight—the results are summarised in Table C.

The arrangement, fig. 1, was made so that the plate C, from its position, was exposed near the bottom at (*a*) to the action of water containing rather more salt than the water adjacent to the top at (*b*), whilst constant diffusion was at the same time proceeding, the salt water from vessel S also coming over into the distilled water at an increasing ratio, by means of the soft moist leather connexion; the water, however, about the position (*b*) nearer the surface, maintained a less saline composition than at (*a*), and the estimation of this difference between top and bottom water during the periodic slow diffusions, is shown in the following analysis, selected as an example from a number of similar observations taken at intervals during the progress of the investigation.

It will be seen that the diffusion cell arrangements allowed equilibrium of composition between bottom and surface to be restored in about fourteen days. As there was easy communication (about ½ inch) around the edges, not only of the plate C, but of the others, the fluid during diffusion in vessel F would probably arrange itself in layers of fairly equal density therein. These diffusion results also

compare favourably with statistics (examined and formerly referred to by the author, and some of which are given in the Appendix) as to differences of composition between surface and bottom waters during diffusion in some portions of such a tidal stream as the Thames, and therefore afford an approximation to effects arising from similar conditions existing during such tidal action; the proportional difference of salinity between top and bottom waters in parts of the Thames during certain states of tide, &c., being, however, sometimes greater than the experimental divergencies given on Table B.

Table B.—Analysis of Water from Diffusion Cell. Results in grains per Gallon.

Time from commencement of each diffusion.	Sodium chloride from position (*a*) near bottom.	Sodium chloride from position (*b*) near surface.	Difference of composition between water about (*a*) and (*b*)
1 day........	1·44	1·32	0·12
2 days.......	2·87	2·64	0·23
3 "	4·31	3·95	0·36
4 "	5·90	5·01	0·89
5 "	7·64	5·81	1·83
6 "	9·38	6·61	2·77
7 "	11·12	7·41	3·71
8 "	22·19	19·42	2·77
9 "	33·26	31·43	1·83
10 "	44·32	43·44	0·88
11 "	50·14	49·79	0·35
12 "	50·73	50·49	0·24
13 "	51·32	51·19	0·13
14 "	51·90	51·90	Nil.

The samples of water for analysis were carefully taken from vessel F by pipette at points indicated on fig. 1 for convenience and accuracy, so as least sensibly to disturb the solution.

The electric action between the plate (which was the positive metal throughout the whole period) in the sea water and that in the other water, was in course of the research ascertained by occasionally introducing in the circuit a delicate galvanometer of known resistance and constants, and the resistance of the cells having been regularly determined, the highest E.M.F., in the case of two representative plates, was found to be about 0·14 volt, which gradually reduced as diffusion proceeded.

The loss by simple corrosion in the sea water alone, as compared with that taking place in the diffusing salt and fresh water, is shown by the relative behaviour of the two plates A and C, of the same

metal in each case, from which it is manifest that a diffusing mixture of salt and fresh water, although containing a comparatively small proportion of saline matter, is capable of setting up an amount of corrosive mischief far greater than that arising from the action of sea water only, the increased loss varying from about 15 up to 50 per cent., according to the nature of the metals. From a reference to column 3 it will also be seen that the combined action of the sea and distilled water (although showing only such a slight saline composition and difference of salinity as indicated on Table B of analysis) whilst diffusing, produced an amount of local galvanic action in the same plate C, sufficient to induce therein much greater corrosion than in the case of plate B, although the latter was constantly immersed in the sea water (containing 2276·8 grains per gallon of sodium chloride), and was at the same time metal positive, as indicated by the galvanometer.

Table C.—Loss by Corrosion of Metals in Sea Water, and of the same Metal exposed to the Galvanic Action of diffusing Salt and Fresh Water during Fifty-two Weeks. Results in grains per square foot of surface exposed.

Description of metals.	Percentage of combined carbon.	Col. 1. Simple corrosion of the plate A in the sea-water vessel S.	Col. 2. Galvanic corrosion of the plate B in the sea-water vessel S.	Col. 3. Galvanic corrosion of the plate C in diffusing sea-water and distilled water.	Col. 4. Corrosion of the plate D in diffusing sea-water and distilled water.	Col. 5. Excess of loss from the action on plate C of diffusing salt and fresh water compared with loss in sea-water only. See column 1.
Common cast metal (No. 2)	0·670	235·42	240·07	289·03	236·75	53·61
"Soft" Siemens-Martin steel	0·170	259·94	262·38	395·52	344·34	135·58
Wrought iron.......	None	270·28	284·16	365·98	310·60	95·70
"Soft" cast steel....	0·460	278·54	343·98	349·29	249·38	70·75
"Soft" Bessemer steel	0·150	300·26	278·92	345·16	252·92	44·90
Best cast metal (No. 1)	0·390	308·68	326·99	354·39	269·61	45·71
"Hard" Bessemer ..	0·510	309·42	304·39	402·31	309·27	92·89

The experiments recorded in this and the former paper indicate therefore, that the tidal action on any vessel or metallic structure of sea and fresh water whilst diffusing, is (in the case even of the same

Table D.

Thames Water, January 15, 1878.								
London Bridge.			Greenwich.			Crossness.		
Depth from surface.	Tide.	Chloride of sodium in water.	Depth from surface.	Tide.	Chloride of sodium in water.	Depth from surface.	Tide.	Chloride of sodium in water.
		Grains per gal.			Grains per gal.			Grains per gal.
Surface	1 hour flood	1·98	Surface	2 hours flood	2·39	Surface	4 hours flood	91·95
Middle	" "	3·46	Middle	" "	2·47	20 ft. 0 in...	" "	109·50
			27 ft. 0 in. .	" "	2·80	40 ft. 0 in...	" "	117·46
Surface	½ hour ebb ..	2·36	Surface	2 hours ebb..	3·95	Surface	½ hour ebb ..	104·48
Middle	" "	2·72	13 ft. 6 in...	" "	3·89	17 ft. 6 in...	" "	110·16
Bottom	" "	2·44	27 ft. 0 in...	" "	4·14	35 ft. 0 in...	" "	213·70
			Surface	3 hours ebb..	3·05	Surface	1½ hours ebb	101·43
			11 ft. 0 in...	" "	4·86	17 ft. 0 in...	" "	130·35
			22 ft. 0 in...	" "	2·93	34 ft. 0 in...	" "	189·18

metal thus exposed to the simultaneous action of top and bottom waters) considerably more destructive in its nature and character than the action of sea water alone.

Moreover, the author has found it (in other experiments extending over long periods) to considerably exceed (in some instances varying from about 55 to 120 per cent.) the loss caused by galvanic action between dissimilar metals of the iron and steel group, in circuit in sea water.

The arrangement of the waters in tidal streams and various effects consequent thereon form an intricate study; the author trusts, however, that the limited observations recorded in the present and the former paper may have afforded some information on the metallurgical aspect of the subject.

Appendix.

The gradual rise and consequent inward flow of salt water and the outward flow of fresh water, has a general tendency to arrange the waters of a tidal stream into long, overlapping, wedge-like, layers or formations, the lower containing denser salt water and the upper more fresh water. This disposition of the waters is modified very considerably by currents, inter-diffusion, and numerous other conditions. The arrangement and diffusion of the salt and fresh water may not necessarily, at all places in the stream, be of an even character, almost isolated bodies of salt and fresh water not improbably accumulating in the numerous creeks, basins, or other indentations along the shores. The general contour of a stream, the influence of rainy or dry seasons (affecting the proportion of fresh water), the fact of its estuary being either long, deep, and narrow, affording little fall, or, on the contrary, of a wide shallow character, the states and times of tide, &c., and many other circumstances, also variously modify the diffusion results.

To illustrate further the basis and nature of this investigation, the author gives on Table D some typical analyses, selected from information relating to the disposition of the waters of tidal streams, kindly furnished by Dr. H. Clifton Sorby, F.R.S.; reference may also be made to the recent very interesting research "On the Salinity of the Firth of Forth," by Mr. Hugh Robt. Mill, B.Sc., F.C.S. (read before the Royal Society, Edinburgh, January 5th, 1885).

II. "The Removal of Micro-organisms from Water." By PERCY F. FRANKLAND, Ph.D., B.Sc., F.C.S., Assoc. R. Sch. Mines, Demonstrator of Chemistry in the Normal School of Science, South Kensington Museum. Communicated by E. FRANKLAND, F.R.S. Received May 18, 1885.

The overwhelming evidence which has been now accumulated of the fact that some at least of the diseases called "zymotic" are propagated by means of living organisms, renders it interesting to discover in what manner such organisms may be removed from the media—air and water—through which they are in general distributed. In the following pages I have the honour to bring before the Royal Society the results of some experiments upon which I have been recently engaged, with a view to discover whether and to what extent micro-organisms may be removed from water by submitting this medium to some of the various processes of treatment which are in vogue for its purification. For although the chemical efficiency of numerous methods of water-purification has been largely studied, little has been done in the matter of determining their value as agents for the removal of micro-organisms.

The method of investigation which I have adopted was to take water in which the number of organisms was approximately known, submit this to treatment in such a manner as not to introduce extraneous organisms during the experiment, and then determine the number of organisms which remained in a given volume of the water after treatment. Since we are doubtless at present only acquainted with a few of the micro-organisms which are capable of producing disease, it appeared to me to be, in the first instance, desirable to study the question irrespectively of the nature of the organisms, and only to take into consideration their aggregate number before and after treatment. Moreover the employment of specific organisms would in all cases have greatly enhanced the difficulty of the experiments, and would in some cases have actually rendered them impossible.

The organisms generally used in these experiments were those which develope in diluted urine after exposure to the air. The solution was further diluted more or less with water so as to obtain a liquid containing a convenient number of organisms in a given volume.

The method of determining the number of organisms present in the waters, both before and after treatment, was, with a few modifications, that devised by Koch, in which a definite volume of water is mixed with sterilised nutritive gelatine and then poured out upon a

glass plate, when, after the lapse of a few days, the colonies derived from the individual centres of life can be counted by means of a lens, and from this the number present in a cubic centimetre or any other volume of water can be calculated. The following is a description of the exact mode of procedure adopted :—

Determination of the Number of Micro-organisms in Water.

The nutritive gelatine was prepared thus :—1 lb. of lean meat is finely minced and then infused with $\frac{1}{2}$ litre of water for 1—2 hours, the solid part being then strained off through linen. 100 grams of white French gelatine is allowed to soak in another $\frac{1}{2}$ litre of water, and to this the extract of meat obtained above is added. The whole is now boiled for a few minutes to complete the solution; 10 grams of peptone and 1 gram of common salt are then added and dissolved. The mixture so obtained gives an acid reaction, which is carefully neutralised with carbonate of soda. The liquid is now clarified by beating in the contents of two or three eggs along with the broken shells, the whole being briskly boiled for a few minutes. The coagulated albumen rises to the surface and carries with it the other solid particles in the liquid. On then straining through linen, an almost clear liquid is obtained, which is finally clarified by passing through filter-paper kept hot by means of a water-jacket. On cooling this liquid, it sets to a yellowish-brown transparent jelly. Whilst still liquid it is poured into clean test-tubes, so that each of these contains 2 or 3 c.c. The test-tubes are tightly plugged with cotton-wool, and then sterilised by steaming them for half an hour, on three consecutive days. Tubes thus prepared were found to keep for an indefinite period of time. The glass plates destined to receive the film of gelatine were well washed, and then placed in a copper box provided with a tightly fitting lid, the whole being sterilised by heating for at least three hours to 150° C.

The glass dishes in which the gelatine plates are placed during the development of the organisms were always well washed, and then rinsed with a 2 per cent. solution of mercuric chloride immediately before use. Some of the same solution of mercuric chloride was poured into the bottom dish, so as to preserve the internal atmosphere saturated with moisture, and the two dishes were always placed in a porcelain tray containing a solution of mercuric chloride, so that the interior of the dishes was disconnected from the outside air by means of a mercuric chloride seal, whereby all ingress of organisms from the atmosphere was prevented during incubation. The dishes also contained a small glass tripod, bearing a glass plate, the surface of which was carefully levelled by placing the dishes on a table provided with three screws. When the apparatus is thus prepared, the sterilised plate is rapidly transferred to the levelled plate, the gelatine is carefully melted in

its test-tube, and before removing the cotton-wool plug the latter is burnt outside, so as to destroy any organisms that may be adhering to the exterior. A given number of drops of the water or other liquid under examination are now introduced into the open test-tube by means of a pipette, which has been previously sterilised by heating it nearly to redness in a Bunsen flame. The water and liquid gelatine are mixed by agitation, and then quickly poured out on to the sterilised plate, the glass cover being immediately replaced. The whole operation is so managed that the time of exposure to the air is reduced to a minimum. A solution of mercuric chloride is then poured into the porcelain tray as already described, and when the gelatine has set, which generally takes place in about ten minutes, the dishes are placed in a cupboard, and maintained at a temperature of 20—25° C.

After a period of incubation, varying from 3—6 days, the organisms make their appearance in isolated colonies, which may be readily counted with the assistance of a lens. The operation of counting is greatly facilitated by placing the plate on a black ground, ruled in squares.

That when due precautions are taken in the execution of this process little or no appreciable error is introduced by the unavoidable but momentary contact of the gelatine with the air, was proved by making blank experiments, in which all the abovementioned operations, excepting the addition of water, were performed, and in these cases no organisms were found. Additional proof of this is also furnished by the fact that on several occasions no organisms were found in the course of the experiments to be described below.

The experiments were in nearly all cases made in duplicate, the concordance in the results of parallel experiments being, on the whole, very satisfactory; occasionally, however, wide discrepancies did occur, but these could in most cases be accounted for through the water under examination not having been rendered sufficiently homogeneous by agitation.

Experiments on the Filtering Power of Different Substances.

The substances selected for experiment were natural greensand, silver sand, powdered glass, brickdust, coke, animal charcoal, and spongy iron.

These substances were all obtained in a fine state of division by powdering them in a mortar, and then passing them through a sieve (40 meshes to the inch). The filters were constructed of pieces of glass tubing (1 inch diameter), drawn off at one extremity to a small aperture; the latter was plugged with a small quantity of asbestos, and upon this was placed a column 6 inches in height of the closely packed filtering material, the surface of which was again protected by

a thin layer of asbestos. Before use, the filter thus prepared was sterilised by heating it to a temperature above 150° C. for at least three hours. After sterilisation the filter was at once put in operation by supplying the infected water at the wide end of the tube, the filtered water being collected when required in a vessel sterilised in the same way beneath the lower extremity of the tube. In those experiments in which the filtration was carried on for many days, or even weeks, the infected water was constantly supplied to the filter by means of an inverted flask fitted with a delivery-tube, the latter dipping into the water above the filtering material. The following results were obtained with the various materials :—

Greensand.—The urinous water passed through this filter, on starting, at the rate of 2·07 inches per hour. The sand was highly ferruginous, and the filtered water contained a noticeable proportion of iron.

On examining the unfiltered water, it was found to contain 64 centres of life in one, and 97 centres per c.c. in a duplicate experiment; these consisted almost wholly of organisms causing liquefaction of gelatine, a few fungi, and the remainder small spherical colonies. In the filtered water, on the other hand, there were no organisms of any description discoverable. *The filtration had thus completely sterilised the water.*

In order to ascertain whether the greensand would continue to exercise this influence, the arrangement for continuous filtration, as already described, was put in operation, and after being in action for thirteen days, during which time 7·1 litres of water passed through, the efficiency of the filter was again tested. The *unfiltered* urine-water was found to contain 8193 centres per c.c. The *filtered* water was found to contain 1071 centres per c.c. These experiments show that although the original power of the greensand was broken down, the filter was still arresting a considerable proportion of the organisms present in the water passing through it.

The efficiency of the filter was again determined after the continuous filtration had proceeded for one month, when 20 litres of water had passed through. The following results were obtained :—

The *unfiltered* water contained	..	1281 centres per c.c.
The *filtered* „ „	..	779 „ „

On the whole, therefore, even after the lapse of an entire month, a notable proportion of the organisms was still being removed by the greensand filter.

Animal Charcoal.—Perfectly similar experiments were made with this well-known filtering material. On examining the initial efficiency of the filter, it was found that whereas the *unfiltered* water contained so many organisms that the gelatine on the plates had become entirely

liquefied, the *filtered* water did not contain in either of two experiments any organism whatever. The filter had thus completely sterilised the water passing through it.

The rate of filtration was considerably less rapid than in the case of the greensand, being only 0·82 inch per hour. Continuous filtration was then carried on for twelve days, during which time 4·2 litres of water passed through, after which the unfiltered and filtered waters were again examined.

The *unfiltered* water contained	..	2792 centres per c.c.
The *filtered* „ „	..	No organisms of any kind.

Thus the efficacy of the filter remained unimpaired after twelve days' continuous action.

After acting for one month, 14·6 litres of water having passed through, the waters were again examined.

The *unfiltered* water contained	..	1281 centres per c.c.
The *filtered* „ „	..	6958 „ „

Thus at the end of one month the filter was actually delivering water more highly impregnated with organic life than that with which it was supplied.

Spongy Iron.—The filtering power of this material was also examined by means of a similar series of experiments. The rate of filtration at the commencement was 1·84 inches per hour, and the following results were obtained :—

Unfiltered water contained	....	80 centres per c.c.
Filtered „ „	....	No organisms whatever.

After twelve days' continuous filtration, 3·6 litres of water having passed through, the following results were obtained :—

Unfiltered water contained	..	2792 centres per c.c.
Filtered „ „	..	No organisms whatever.

Again, at the close of one month's continuous filtration, 9 litres of water having passed through, the examination was repeated :—

Unfiltered water contained	..	1281 centres per c.c.
Filtered „ „	..	2 „ „

The column of spongy iron, 6 inches in depth, was thus able to remove all the organisms from the water for upwards of twelve days, and even at the end of the month, the water after filtration was almost destitute of organic life. The rate of filtration was, however, greatly diminished in the course of the month during which the filter was in operation. It is worthy of notice that the organisms found in the filtered water were all of the same kind, and caused no lique-

faction of the gelatine, whilst a number of those in the unfiltered water produced liquefaction of this medium.

Brickdust.—The following results were obtained with a filter constructed similarly to the above, and charged with pulverised red brick, the powder being passed through the before-mentioned sieve.

Initial efficiency.

Unfiltered water contained..	3112 centres per c.c.	
Filtered ,, ,, ..	732 ,, ,,	

After being in action for five weeks, during which time 12·75 litres of water passed through the filter—

Unfiltered water contained..	5937 centres per c.c.
Filtered ,, ,, ..	406 ,, ,,

This material, therefore, does not wholly remove the organisms, even when fresh, but it continues, even after five weeks, to remove a considerable proportion of them.

Coke.—With this material, which was also used in the same state of division, the following results were obtained:—

Initial efficiency.

Unfiltered water contained..	3112 centres per c.c.
Filtered ,, ,, ..	No organisms whatever.

After being in action for five weeks, when 13·25 litres of water had passed through—

Unfiltered water contained..	5932 centres per c.c.
Filtered ,, ,, ..	86 ,, ,,

This substance, therefore, possesses filtering powers which are second only to those of spongy iron; at the outset the result is equally perfect with both, and even at the close of five weeks' continuous work the coke removes a large proportion of the micro-organisms present in the unfiltered water. Moreover, the rate of filtration through coke was considerably greater. In the case of the coke, as in that of the spongy iron, the organisms found in the filtered water were almost exclusively of one kind, only one colony causing liquefaction, forming an exception.

Silver Sand.—Owing to the highly ferruginous character of the greensand employed in the previous experiments, and bearing in mind the well-known antiseptic properties of salts of iron, it was deemed advisable to investigate the filtering power of sand free from iron. The silver sand employed for this purpose was digested for some days with hydrochloric acid, the latter then carefully washed out, and the filter sterilised as already described. The following results were obtained with this material:—

The rate of filtration was exceedingly rapid, viz., 1 inch in less than two minutes, or more than 30 inches per hour. The number of organisms found in the unfiltered and filtered waters respectively were—

Unfiltered water......	11,232	centres per c.c.
Filtered „	1,012	„ „

Thus already at the outset the silver sand filter fails to arrest all the organisms in the water, although it very considerably diminishes their number, even when the rate of filtration is exceedingly rapid.

Powdered Glass.—This material also was employed, with a view to determining the value of non-ferruginous siliceous matter. The rate of filtration in this case was also very great, viz., 1 inch in less than eight minutes.

The following results were obtained :—

Unfiltered water	11,232	centres per c.c.
Filtered „	792	„ „

The results obtained with the powdered glass very closely resemble those obtained from the silver sand; but as neither were efficient filters, even at the outset of their action, no further experiments were made with them.

Removal of Micro-organisms by Agitation with Finely Divided Solid Matter.

The above experiments show that micro-organisms may be more or less completely removed by mere contact with solid particles, some of which are incapable of exerting any chemical action upon them; it therefore appeared of interest to ascertain whether these organisms may be removed by agitating the water with the same substances, and then allowing them to subside. The experiments undertaken with this object were conducted in the following manner:—

50 c.c. of urine-water, in which the number of organisms was determined, were placed in a small accurately stoppered bottle, previously sterilised by being heated to 150° C., for at least three hours. A given weight of the substance was placed in a small test-tube, plugged with cotton-wool, and then sterilised in the same way. The sterilised substance was then transferred to the water in the stoppered bottle, and the latter violently shaken for a definite length of time, after which it was allowed to stand at rest until complete subsidence had taken place. Some of the clear supernatant liquid was then taken out with a pipette, and examined for organisms by means of gelatine cultivation in the manner already described. The substances were in all cases employed in the same state of division as in the filtration experiments.

Spongy Iron.—5 grams of this substance were shaken up with 50 c.c. of urine-water in one case for one minute, and in another for fifteen minutes; in both cases the waters were subsequently allowed to subside for half an hour before examination. The following results were obtained:—

Untreated water	....	609 centres per c.c.
Treated ,,	shaken for 1 minute.	28 ,, ,,
,, ,,	,, ,, 15 minutes	63 ,, ,,

Thus the reduction in the number of organisms by agitation is exceedingly marked, and curiously the most favourable result was obtained when the agitation was only continued for a single minute. A pure cultivation was not obtained in the treated water, some colonies producing liquefaction of gelatine, and others not, thus presenting a contrast to the results obtained by filtration through this material.

Chalk.—1 gram of water was shaken up with 50 c.c. of urine-water for fifteen minutes, and then allowed to subside for five hours. The following results were obtained :—

Untreated water	8325 centres per c.c.
Water after 15 *minutes' agitation with chalk*	274 ,, ,,

A very large reduction indeed in the number of organisms was thus obtained.

Animal Charcoal.—1 gram of animal charcoal was shaken up with 50 c.c. urine-water for fifteen minutes, and then allowed to subside for nearly five hours. The following results were obtained :—

Untreated water	8325 centres per c.c.
Water after 15 *minutes' agitation with animal charcoal*.....................	60 ,, ,,

The efficiency of the animal charcoal in this respect is thus very markedly greater than that of chalk.

Coke.—1 gram of coke was shaken up with 50 c.c. of urine-water, and then allowed to subside for forty-eight hours, as the water did not clear before. The following results were obtained:—

Untreated water	Too numerous to be counted.
Water after 15 *minutes' agitation with coke*....................	No organisms whatever.

It thus appears that simple agitation with coke for fifteen minutes is sufficient to entirely remove all organisms.

China Clay.—1 gram of this was shaken for fifteen minutes with 50 c.c. of urine-water; subsidence was not complete for five days. On examining the clear water it was found to yield a very large number of organisms indeed, thus showing that prolonged subsidence with

finely divided matter like clay is not conducive to the separation of micro-organisms.

A similar result was obtained with brickdust, which, however, did not take quite so long to subside.

Effect of Subsidence on Micro-organisms in Water.

As in the above agitation experiments the water for examination was always taken from the clear upper layers, it became of interest to know whether the micro-organisms would not, by subsidence alone, separate out from the upper layers without the influence of solid particles. In order to ascertain this, three sterilised Winchester bottles were filled up to the shoulder with urine-water, and plugged with sterilised cotton-wool. The bottles were placed in a room (temperature about 10° C.) and left at perfect rest; the number of organisms in the urine-water was ascertained at the outset of the experiment, again at the end of six hours the number was determined in one of the bottles, at the end of twenty-four hours in the second bottle, and lastly, at the end of forty-eight hours in the third bottle. The numbers found were as follows:—

Hours of subsidence.	No. of centres found per c.c. of water.
0	1,073
6	6,028
24	7,262
48	48,100

These experiments show that far from there being any tendency for the upper layers of water to become deprived of organisms by subsidence, the tendency is for the number to increase very rapidly indeed.

Effect of Clark's Process on Micro-organisms in Water.

Owing to the encouraging results obtained by agitating water with finely divided chalk, it appeared probable that still more striking effects would be obtained if the chalk were present in a more finely divided state, such as is the case when water is softened by means of lime (Clark's process). In order to ascertain the effect of Clark's process in this respect, three stoppered Winchester bottles were taken, and to each were added 2 litres of ordinary Thames water, to which some urine-water had been added, so as to impart a convenient number of organisms. To two of these bottles 100 c.c. of clear lime-water (1 c.c. = 0·0013 gram CaO) were added, calculated to remove 11·6 parts of carbonate of lime per 100,000 parts of the water. Each of these bottles was violently shaken, and then allowed to subside for eighteen hours. The third bottle, to which no lime-water was added,

was first tested for the number of organisms contained in the water used in the experiment. After eighteen hours the two bottles to which lime-water had been added were tested without disturbing the precipitate, and also the third bottle containing the untreated water which had been left at rest in the same place as the other two. The following results were obtained:—

Untreated water	85	centres	per c.c.
„ „ *after* 18 *hours' rest*	1,922	„	„
Water after Clark's process and 18 *hours' subsidence*	42	„	„

In order to appreciate the real effect of the treatment by Clark's process, it is necessary that the treated waters should be compared, not with the original water, but with the latter after eighteen hours' rest, for this shows what the condition of the water would have been at the time of testing if no lime-water had been added. It is evident that after the subsidence of the carbonate of lime precipitate has taken place, there is every probability of the organisms becoming again distributed throughout the upper layers of the water, and with a view of determining whether this actually takes place or not, the same waters which had remained well stoppered and at rest were again tested after the lapse of ten days. It was then found that the untreated as well as the softened waters contained immense numbers of organisms in their upper layers.

As the effect of Clark's process in removing organisms from water appeared to be of great practical importance, the above experiments were repeated, the conditions being essentially the same as before. The following results were obtained:—

Untreated water................	37	centres	per c.c.
„ „ *after* 21 *hours' rest*	42	„	„
„ „ *after* 48 *hours' rest*	298	„	„
Water after Clark's process and 21 *hours' subsidence*	22	„	„
Water after Clark's process and 48 *hours' subsidence*	166	„	„

Owing to the number of organisms in the original water having been very much smaller, the results are not so pronounced as in the former case, the main facts are, however, fully substantiated.

It appeared also to be of interest to ascertain what results are obtainable on the large scale. For this purpose the process of softening as practised at the Colne Valley Waterworks at Bushey, near Watford, was investigated, as well as the new modification of Clark's process devised by Messrs. Gaillet and Huet, which is now in operation at Mr. Duncan's, Clyde Wharf, Victoria Dock. I am indebted

to Mr. Verini, of the Colne Valley Waterworks, as well as to Mr. Duncan and Mr. Newlands, for their kindness in permitting me to carry out these experiments.

At the Colne Valley Waterworks, the hard water obtained from a deep well in the chalk is mixed with the requisite proportion of clear lime-water, and then allowed to settle in open tanks. The subsidence is so rapid that under favourable circumstances the upper layers of water are, after three hours' time, fit for distribution. On the occasion of my visit, however, boring operations were being carried on, and the water was in consequence milky, and the necessary subsidence after softening had to be increased to two days. I was unfortunately unable to obtain a perfectly representative sample of the water before softening, and the number of organisms found in the untreated water is probably in excess of that which was actually present in the unsoftened water itself. The following results were obtained :—

Unsoftened water..............	322 centres per c.c.
Water after softening and 2 days' subsidence (from main)	4 „ „

The almost complete absence of organisms in the softened water shows how perfect a result is obtained even on the large scale.

In the process of softening, due to Messrs. Gaillet and Huet as practised at Mr. Duncan's, the water from an artesian well is mixed with a suitable proportion of lime-water and caustic soda, the mixture being then made to pass upwards through a tower provided with diaphragms, which accelerate the precipitation of the carbonate of lime. The passage through this tower occupies a period of about two hours. Samples of water before and after treatment were examined with the following results :—

Well water	182 centres per c.c. (38 caused liquefaction of gelatine.)
„ *after softening*	4 centres per c.c. (None of the centres caused liquefaction of gelatine.)

These experiments, as well as those made in the laboratory, show that the softening of water by Clark's process is attended with a great reduction in the number of organisms, the best results being obtained when the clear liquid is separated from the precipitated carbonate of lime as speedily as possible.

Pasteur's Filter.—Through the kindness of Colonel Sir Francis Bolton, R.E., I have had the opportunity of examining one of the above filters, in which the water is made to pass through a cylinder of biscuit-porcelain. The one with which my experiments were made

consisted of ten such cylinders, and the water (ordinary Thames water) was forced through under a pressure of between 30 and 40 feet of water. Under these circumstances the filter commenced by yielding 1 litre in 40 minutes, or 36 litres per 24 hours, but already at the end of a fortnight's continuous action it was only delivering 1 litre in 1 hour 14 minutes, or rather less than 20 litres per 24 hours; and after $2\frac{1}{2}$ months the rate of filtration was 1 litre in 1 hour 22 minutes, or $17\frac{1}{2}$ litres in 24 hours.

The water both before and after filtration was examined for micro-organisms with the following results:—

Thames water..............	54 centres per c.c.
„ *after filtration*..	0

The water before and after filtration was also submitted to chemical analysis with the following result:—

Results of Analysis expressed in Parts per 100,000.

	Thames water.		
	Before filtration.		After filtration.
Total solid matters............	33·70	....	30·04
Organic carbon	·282	....	·284
„ nitrogen	·028	....	·027
Ammonia	0	....	0
Nitrogen as nitrates and nitrites	·288	....	·289
Total combined nitrogen	·316	....	·316
Chlorine	1·9	....	1·9
Temporary hardness	15·7	....	14·4
Permanent „	4·9	....	5·3
Total „	20·6	....	19·7

Both samples were clear and palatable.

It thus appears that although this filter, when new, effects the complete removal of the micro-organisms in the water, it has but a very trifling influence upon the chemical composition of the water, the only change in this respect being a slight diminution in the amount of mineral matter present.

Micro-organisms in Potable Water.

I have also submitted numerous samples of natural waters of different origin to examination for the number of micro-organisms which they contain. My investigation in this direction is, however, still far from complete, but I append the results which I have obtained from a monthly examination of the various waters supplied to the Metropolis during the present year. When the history of the water is accurately

known, and due precautions in collecting samples have been taken, there can be no doubt that in many cases it is capable of throwing considerable light upon the quality of water and in assisting to interpret the results of chemical analysis.

The method of collecting samples which has been employed by me is the following:—

Small (about 3 oz.) bottles, accurately stoppered, and sterilised by being heated to 150° C. for at least three hours, are kept tightly stoppered until they are to be used. In taking the sample, the outside of the bottle is well washed in a stream of the water to be examined, the stopper is then removed, the bottle nearly filled with water, and the stopper replaced as rapidly as possible. The examination of the water should follow as soon as possible after collection.

The following results were obtained with samples taken as above from the mains of the various companies supplying London:—

	No. of centres per c.c. of water.		
	January.	February.	March.
Chelsea	8 (0 liq.)*	23 (2 liq.)	10 (0 liq.)
West Middlesex	2 (0 liq.)	16 (2 liq.)	7 (0 liq.)
Southwark	13 (0 liq.)	26 (2 liq.)	246 (1 liq.)
Grand Junction	382 (4 liq.)	57 (23 liq.)	28 (12 liq.)
Lambeth	10 (2 liq.)	5 (0 liq.)	69 (1 liq.)
New River	7 (4 liq.)	7 (0 liq.)	95 (1 liq.)
East London	25 (0 liq.)	39 (0 liq.)	17 (0 liq.)
Kent	10 (0 liq.)	41 (0 liq.)	9 (1 liq.)

* Liq. denotes that the organisms caused liquefaction of the gelatine.

It would be premature to draw any conclusions from these results, and I purpose to continue these observations over a longer period of time.

The waters were also at the same time submitted to chemical analysis, so that their biological and chemical characters might be compared; the results are given below.

General Conclusions.—(1.) Of the substances experimented with, only greensand, coke, animal charcoal, and spongy iron were found to wholly remove the micro-organisms from water filtering through them, and this power was in every case lost after the filters had been in operation for one month. With the exception of the animal charcoal, however, all these substances, even after being in action for one month, continued to remove a very considerable proportion of the organisms present in the unfiltered water, and in this respect spongy iron and coke occupy the first place.

Results of Analysis expressed in Parts per 100,000.

London Waters.	Temperature in Centigrade degrees.			Total solid matter.			Organic carbon.			Organic nitrogen.			Ammonia.	Nitrogen as nitrates and nitrites.		
	Jan.	Feb.	Mar.	Jan.	Feb.	Mar.	Jan.	Feb.	Mar.	Jan.	Feb.	Mar.	Jan., Feb., and Mar.	Jan.	Feb.	Mar.
Thames.																
Chelsea........	3°·8	5°·9	6°·2	30·78	31·04	30·24	·166	·188	·192	·017	·017	·033	0	·251	·272	·242
West Middlesex.	4·0	6·8	7·5	30·50	30·40	30·60	·171	·210	·218	·028	·021	·031	0	·267	·275	·253
Southwark	4·4	6·8	7·5	31·16	29·74	30·04	·221	·286	·181	·053	·025	·030	0	·291	·283	·241
Grand Junction.	2·8	6·0	6·8	27·90	30·14	30·46	·255	·314	·150	·043	·033	·023	0	·181	·278	·251
Lambeth	3·7	6·4	6·9	25·50	27·26	30·50	·181	·179	·174	·046	·019	·021	0	·181	·219	·256
Lea.																
New River	3·4	5·9	6·8	31·80	31·32	28·14	·085	·080	·100	·015	·012	·019	0	·326	·361	·272
East London ...	3·8	6·0	7·2	35·54	36·70	34·90	·192	·200	·200	·043	·019	·034	0	·341	·402	·327
Deep Wells.																
Kent	12·0	12·0	12·6	41·36	35·40	37·12	·039	·024	·033	·010	·006	·006	0	·463	·416	·423

(2.) The results obtained by agitating water with various solid materials show that a very great reduction in the number of suspended organisms may be accomplished by this mode of treatment, and the complete removal of all organisms by agitation with coke is especially worthy of notice.

(3.) Again, the results obtained with Clark's process show that we possess in this simple and useful mode of treating water a means of greatly reducing the number of suspended organisms.

(4.) Thus, although the production in large quantity of sterilised potable water is a matter of great difficulty, involving the continual renewal of filtering materials, there are numerous methods of treatment which secure a large reduction in the number of organisms present.

Moreover, in judging of the value of filtering materials from examinations of this kind, it is only reasonable that a preference should be given to those materials with which a practically pure cultivation is obtained in the filtrate over those materials which appear to exercise no selective action upon micro-organisms.

In conclusion, I would point out that it is very desirable that experiments of this kind should be greatly multiplied and repeated under varying conditions, and it is my intention to continue and extend this examination.

III. "A Memoir introductory to a General Theory of Mathematical Form." By A. B. KEMPE, M.A., F.R.S. Received May 18, 1885.

(Abstract.)

The memoir is divided into 426 short sections which are arranged under 42 heads. Each head is given in the abstract, with a brief reference to the nature of the sections it comprises, except in the case of the second head, viz., "Fundamental Principles," the sections under which are given almost in full.

§§ 1—2. *Scope of the Memoir.*

The object of the memoir is the treatment of the "necessary matter" of exact or mathematical thought as a connected whole; the separation of its essential elements from the accidental clothing—algebraical, geometrical, logical, &c.—in which they are usually presented for consideration; and the indication of that to which the infinite variety which those elements exhibit is due.

The memoir is introductory only, comprising the statement of fundamental principles, and the vindication of their truth by a sufficient variety of applications.

§§ 3—13. *Fundamental Principles.*

(3.) Whatever may be the true nature of things and of the conceptions which we have of them (as to which points we are not concerned in the memoir to inquire), in the operations of reasoning they are dealt with as a number of distinct entities or *units*.

(4.) These units come under consideration in a variety of guises—as points, lines, statements, relationships, arrangements, intervals or periods of time, algebraical expressions, &c., &c.—occupy various positions, and are otherwise variously circumstanced. Thus while some units are undistinguished from each other, others are by these peculiarities rendered distinguishable. For example, the angular points of a square are distinguishable from the sides, but are not distinguishable from each other. In some instances where distinctions exist they are ignored as not material. Both cases are included in the general statement that some units are distinguished from each other and some are not.

(5.) In like manner some *pairs* of units are distinguished from each other while others are not. Pairs may be distinguished even though the units composing them are not. Thus the angular points of a square are undistinguishable from each other, and a pair of such points lying at the extremities of a side are undistinguishable from the three other like pairs, but are distinguishable from the two pairs formed by taking angular points at the extremities of a diagonal, which pairs again are undistinguishable from each other. Further, a pair, *ab*, may sometimes be distinguished from a pair, *ba*, though the units *a* and *b* are undistinguished. Thus if *a*, *b*, *c*, be the angular points of an equilateral triangle, and barbs be drawn on the sides pointing from *a* to *b*, from *b* to *c*, and from *c* to *a* respectively, the angular points *a*, *b*, *c* will be undistinguished from each other, each has an arrow proceeding from it and to it; but the pair *ab* is distinguished from the pair *ba*, for an arrow proceeds from *a* to *b*, but none from *b* to *a*.

(6.) It will be convenient to speak of *ab* and *ba* as different *aspects* of the *collection* of two units, *a*, *b*. Here the terms "aspect" and "collection" are each to be understood as referring to two separate units, and not to those units regarded in the aggregate as a single unit.

(7.) Again we have also distinguished and undistinguished *triads*, *tetrads*, ... *m-ads* .... *n-ads* ....; every *m*-ad being, of course, distinguished from every *n*-ad. Just as we may have *ab* distinguished from *ba*, so we may have an *n*-ad *pqrst* .... *uv* distinguished from *qusvt* .... *rp*. Here *pqrst* .... *uv* and *qusvt* .... *rp* will be termed, as in the case of pairs, different *aspects* of the *collection* *p*, *q*, *r*, *s*, *t*, ... *u*, *v*; the term "collection" being understood to

refer to a number of separate units without reference to the various "aspects" of the collection. Different aspects of the same collection of n units will be spoken of as different n-ads.

(9.) Every collection of units has a definite *form*, due—(1) to the number of its component units, and (2) to the way in which the distinguished and undistinguished units, pairs, triads, &c., are distributed through the collection. Two collections of the same number of units, but having different *distributions* will be of different forms. The angular points of a cube and of an octagon furnish examples of two systems of eight units, having different distributions. In the former case there are three sorts of pairs, in the latter four.

(10.) Two collections which are undistinguished will be of the same form, but two collections which are of the same form are not necessarily undistinguished.

(11.) Each of the forms which a system of n units can assume owing to varieties of distribution is one of a definite number of possible forms, and the peculiarities and properties of the collection depend, as far as the processes of reasoning are concerned, upon the particular form it assumes, and are independent of the dress—geometrical, logical, algebraical, &c.—in which it is presented; so that two systems which are of the same form have precisely the same properties, although the garbs in which they are severally clothed may by their dissimilarity lead us to place the systems under very different categories, and even to regard them as belonging to "different branches of science."

(12.) It may seem in some cases that other considerations are involved besides "form," but it will be found on investigation that the introduction of such considerations involves also the introduction of fresh units, and then we have merely to consider the form of the enlarged collection.

(13.) In order to put form in evidence some "accidental" clothing is of course necessary; if, however, we employ more than one species of clothing, each species being uniform and suited to forms of every kind, the disturbing effect will be reduced to a minimum.

§§ 14—17. *Units.*

The variety of the units which may be dealt with in an investigation is here indicated; *e.g.*, we may have a material object dealt with as one unit, a quality of that object as another, a position it occupies in space as a third, and a statement about it as a fourth. The difficulty of specifying the units considered is referred to, and cases are discussed in which mistakes are likely to occur.

§§ 18—24. *Some Definitions.*

A collection of units selected from a larger collection is termed a *component* of the latter.

A collection of units containing some units selected from each of a number of other collections is said to *connect* the latter.

There are other definitions which need not be referred to.

§§ 25—36. *Systems.*

A collection of units which is such that every component unit is distinguished from every unit which is not a component is termed a *system*. A *single system* is one in which all units are undistinguished. We have also *double*, *treble*, *&c.*, *systems*. The sections contain general observations about systems.

§§ 37—49. *Heaps—Graphical Representation of Units.*

We may graphically represent units by small circles of different colours, spots, &c. These are termed *graphical units.*

A large class of systems called *heaps* may be represented by means of these alone, without further graphical adjuncts.

A *single heap* is one which is graphically represented by graphical units all undistinguished from each other.

A *discrete heap* is one which is graphically represented by graphical units which are all distinguished from each other.

There are intermediate forms called *double*, *treble*, *&c.*, *heaps.*

§§ 50—72. *Pairs—Graphical Representation.*

The three different forms which pairs can assume are considered, viz. :—

(1.) The two units a, b, may be distinguished from each other.

(2.) The two units may be undistinguished but unsymmetrical, *i.e.*, ab distinguished from ba.

(3.) The two units may be undistinguished and symmetrical, *i.e.*, ab undistinguished from ba.

Certain modes are discussed of distinguishing pairs of graphical units, so as to make a graphical diagram represent a system the form of which depends upon pairs being distinguished. These involve the use of plain lines, or "*links*," joining pairs of graphical units; also, where necessary, lines of various sorts, coloured, wavy, dotted, &c.; and, where unsymmetrical pairs have to be represented, the use of barbs on the lines.

§§ 73—83. *Aspects.*

These sections are devoted to a consideration of the real nature of aspects of collections of units, and of the units which are dealt with

when two undistinguished aspects are regarded as corresponding unit to unit. The possibility of representing any conceivable system by a diagram consisting of graphical units and plain lines, or links, only is shown.

§§ 84—86. *Letters, their Sorts and Positions.*

In arrays or other assemblages of letters each letter is of a particular *sort*, and occupies a *position* which is of a particular *sort*. The nature of the collections of units here dealt with is discussed, and they are shown to be the same as those considered in the case of aspects of systems in general.

§§ 87—88. *Representation of Aspects of Collections by Arrays of Letters.*

Hence the propriety of doing that which is indicated by the heading of these sections.

§§ 89—99. *Elementary Properties of Aspects.*

A variety of propositions about aspects are here given, which are needed in subsequent parts of the memoir. Perhaps the most important is the following:—

"If *abcd* .... is undistinguished from *pqrs* ...., and if l, m, n, o .... be units other than $a, b, c, d, \ldots$, there must be units w, x, y, z, ... other than p, q, r, s, ... such that *abcd* .... *lmno* .... is undistinguished from *pqrs* .... *wxyz* ...."

It is pointed out that if any aspect of a whole system S be given, and all other aspects of S which are undistinguished from the given aspect, the form of S is completely determined.

§§ 100—111. *Tabular Representation of Systems.*

Hence we get a simple and uniform method of representing any system, viz., that of arranging the arrays of letters representing the undistinguished aspects one above another in rows, so that letters occupying "the same position" in the different rows all lie in the same column. In the resulting *table* the order of the rows or columns is immaterial. Various points about these tables are considered. A table representing a single heap of n units will have $\underline{|n}$ rows; and one representing a discrete heap of n units will have one row only. In the case of any other form of a system of n units the number of rows will be intermediate between 1 and $\underline{|n}$.

§§ 112—129. *Correspondences of Undistinguished Collections.*

These sections go somewhat fully into the correspondences of undistinguished collections indicated by the tabular representation of a system. The nature of the correspondences where two component

systems of a double system are *independent*, is considered; and the relation of the systems, which are such that one is derived from the other by ignoring differences, is discussed.

§§ 130—136. *Sets.*

Sets are collections of units akin to systems (which are always sets), but may differ from them in each being one only of a number of undistinguished sets.

§§ 137—142. *Aspects Unique with respect to Collections.*

If the aspects xyz .... abc .... and uvw .... abc .... are undistinguished from each other, the aspects xyz .... and uvw .... may be said to be *duplicates* with respect to the collection a, b, c .... If there is no duplicate of xyz .... with respect to a, b, c ...., then xyz .... may be said to be *unique* with respect to a, b, c, ...

§§ 143—151. *Associates.*

(16.) If a, b, c .... be any collection of units, and if λ be another unit, such that the pairs $\lambda a, \lambda b, \lambda c$, ... are distinguished from the pairs which λ makes with units which are not components of the collection, then λ may be said to be an *associate* of the collection a, b, c ....

Many of our conceptions and definitions of systems of units involve the idea of associates.

§§ 152—161. *Unified Aspects.*

These are the units arrived at by regarding the aspects of collections as single units. They are associates of the collections of which they are unified aspects, and are unique with respect to them.

§§ 162—169. *Correspondences of Collections which are distinguished but of like Forms.*

The nature of these correspondences, and the relations which they bear, when regarded as units, to the units of the collections are considered.

§§ 170—173. *Replicas.*

Two systems are *replicas* of each other when they are of the same form and bear precisely similar relations to all other collections of units.

§§ 174—188. *Independent and Related Systems.*

The conditions which must be satisfied in order that systems may not be independent are here investigated, and systems which are *factors* of others are defined and discussed.

§§ 189—193. *Three Modes of Compounding Systems.*

These are selected as illustrative of the way in which certain forms can be derived from others.

§§ 194—195. *A General Method of Graphically representing a Complete System.*

This is effected by the use of graphical units and links only. (See §§ 73—83.)

§§ 196—210. *Networks.*

We here turn to the consideration of systems of pairs, and the *networks* they compose.

§§ 211—221. *Chains.*

A *simple chain* is a succession of undistinguished pairs, *ab*, *bc*, *cd*, *de* .... It may be *open*, *i.e.*, having terminal units, or *closed.* We may also have *compound chains* containing distinguished pairs. The formation of equations by considering coterminous chains is shown.

§§ 222—239. *Groups—Circuits.*

In a *group* each unit is unique with respect to each of the others. The pairs divide up into systems of closed simple chains called *circuits*. Groups give rise to simple associative algebras.

§§ 240—252. *Groups containing from One to Twelve Units.*

In these sections the graphical and tabular representations are given of all groups which have less than thirteen units.

§§ 253—255. *Some General Forms of Groups.*

We have here merely some obvious generalisations of certain forms given under the preceding head.

§§ 256—269. *A Family of Groups.*

The investigation contained in these sections was suggested by a paper by the late Professor Clifford "On Grassman's Extensive Algebra."

§§ 270—274. R-*adic Groups.*

These are such that the aspects of the component collections of R units, when regarded as units, compose a group. (See §§ 362—380.)

§§ 275—279. *Substitutions.*

In considering an array of n letters admitting of certain substitutions, we are considering a system of n units of a definite form.

§§ 280—312. *Algebras.*

The genesis of algebras is here treated of. We have three systems, replicas of each other, of which the units are respectively multipliers, multiplicands, and products. We have also a system the units of which are *primitive equations,* such as

$$ab = c,$$

arrived at by regarding as units the triads of which one unit is a multiplier, one a multiplicand, and one a product. We have another system, of which the units are *primitive algebras,* arrived at by regarding as units certain collections of primitive equations. The derivation of compound algebras is considered, and the way in which algebras arise from special systems.

§§ 313—327. *Quadrates.*

Quadrates are units of a special form of system which may be represented by n asterisks and $n^2 - n$ dots arranged in n rows and n columns, there being $n-1$ dots and one asterisk in each row and each column, and the order of the rows and columns being immaterial. Systems of this form give rise to every species of linear associative algebra. Quadrates are considered by Professor Pierce in his annotations of his father's memoir on "Linear Associative Algebras."

§§ 328—331. *Isolated Collections—Residuals—Satisfied Collections.*

§§ 332—343. *Some Isolated Triad Systems—Family No.* 1.

§§ 344—349. *Some Isolated Triad Systems—Family No.* 2.

We have here a discussion of some interesting systems composed of *isolated collections, i.e.,* collections which are such that each component unit is unique with respect to the rest of the collection.

Systems of Family No. 2 give rise to *self-distributive* algebras, *i.e.,* algebras such that

$$a \,.\, bc = ab \,.\, ac.$$

§§ 350—351. *Geometry in General.*

In most geometrical investigations the units compose a system of a high order of multiplicity; we have points, straight lines, conics, &c., unified collections of these, operators such as quaternions, &c., &c. It is, however, thought to be sufficient, for the purpose of illustration, to refer briefly to some comparatively simple systems.

§§ 352—361. *System of Points and Straight Lines.*

We have here a double system. The pairs connecting the two

systems are of two sorts, viz., a point may lie on or off a line. We may therefore graphically represent such a system by using two sorts of graphical units, a pair of which, one of each sort, are either connected by a link or not, according as the point represented by one lies on the line represented by the other or not. The laws regulating the distribution of the links are stated, and the various forms which collections of three and four points and collections of three lines can assume are considered.

§§ 362—375. *Collinear Points.*

From the results of the sections under the preceding heading the form of a system of collinear points is deduced. It is shown to be a triadic group. The forms of tetrads of points of the system, both harmonic and anharmonic, are discussed.

§§ 376—380. *Ordinary Algebra.*

From the preceding sections we naturally pass to the ordinary algebraic treatment of collinear points, and the nature of quantities.

§§ 381—387. *Coplanar Points and Lines.*

These admit of discussion as a double system. The distribution of the connecting pairs of the two systems is defined, and the algebraic treatment deduced.

§§ 388—389. *Coplanar Points, Lines, and Conics.*

These sections refer briefly to the nature of the treble system composed of coplanar points, lines, and conics.

§§ 390—426. *Logic.*

The memoir concludes with the consideration of the forms of systems of which the units are classes. The investigation leads to a recognition of the fact that on certain points a modification of the views held by modern logicians is necessary.

IV. "On the Influence of Temperature on the Heat of Dissolution of Salts in Water." By WILLIAM A. TILDEN, D Sc. Lond., F.R.S., Professor of Chemistry in the Mason College, Birmingham. Received May 18, 1885.

The experiments described in this paper were undertaken in the hope of obtaining some light upon the vexed question of the condition in which a solid exists when dissolved in a liquid, and were directed more especially towards the investigation of the well-known remarkable phenomena exhibited by sodium sulphate.

Crystallised sodium sulphate melts at about 34°, and at this temperature the salt begins to show signs of dissociation by depositing the anhydrous compound. According to Wiedemann,* indeed, indications of change are observable at a few degrees lower. He finds by observation of the volume of the crystallised salt at different temperatures, that whereas slight expansion occurs from 17° to about 30°, contraction then sets in, at first slowly, then, at 33° to 34°, very rapidly, till the salt melts. The melted salt expands regularly in proportion as the temperature is raised to near 100°. Nicol† has made experiments upon the expansion of solutions of the same salt, and with similar results.

The solubility of sodium sulphate increases rapidly from ordinary temperatures up to 34°, when it attains a maximum. From this point upwards the solubility again diminishes very rapidly to between 40° and 50°, and then less rapidly, till at 100° to 160° it becomes nearly stationary. At temperatures higher than 160° the solubility slowly increases again.‡

In 1874 it was observed by De Coppet§ that heat is developed by contact of water with anhydrous sodium sulphate at temperatures considerably above 34°, at which point the ordinary hydrate is broken up. This circumstance was attributed by Thomsen to the formation of a monohydrate, $Na_2SO_4.H_2O$, and he ascribed this composition to the crystals which are deposited on heating a concentrated solution of sodium sulphate, and which had always been believed to be anhydrous. Thomsen's assumption has, however, been shown to be groundless,|| and therefore the fact noticed by De Coppet remains without adequate explanation.

Below the temperature of 34° it seems probable that the sodium sulphate dissolved in water is in the state of hydrate, $Na_2SO_4 10H_2O$. In what condition is it at temperatures higher than 34°? In other words, does the solution contain the usual hydrate or the anhydrous salt, or both?

This is the question which I have endeavoured to attack by comparing the thermal changes attending the act of solution of the anhydrous salt in water at temperatures above this critical point with the corresponding change at temperatures below. Some interesting results have been obtained, but they do not supply a final answer to this question. My few earlier experiments, communicated in a short note to the British Association at Montreal, gave erroneous results, partly

* "Pogg. Ann.," 1882, 561.

† "Ber. Deut. Chem. Ges.," xv, 1931 *b*.

‡ Tilden and Shenstone, "Phil. Trans.," 1884, I, 23.

§ "Comp. Rend.," 79, 167.

|| De Coppet, "Ber. Deut. Chem. Ges.," xii, 248, and Pickering, "Jour. Chem. Soc.," 1884, 689.

because I was then unaware of the existence of the allotropic modifications of anhydrous sulphate of sodium since discovered by Mr. Pickering (*loc. cit.*), and partly from errors of experiments, which have since been corrected.

The apparatus I have used consists of two parts, the calorimeter and the constant temperature bath. The calorimeter is a brass drum 9 cm. high and 7 cm. diameter, having a paddle which works in a socket in the centre of the vessel, and the spindle of which passes through the lid. At the top of the spindle is a reel, round which passes a silk cord, the ends of which are weighted and drawn over pulleys placed at a little distance on opposite sides. By gently pulling the cord at either end the paddle is made to spin round. Immediately above the blades of the paddle is a wide brass tube perforated with holes, and the bottom of which is immersed in the water, which is placed in the calorimeter. In this tube is placed a thin glass bulb containing the weighed quantity of salt and sealed at the blowpipe. Standing in the tube is a thin brass rod having a fork at the end to act as pulveriser, the top which projects through the lid of the calorimeter; being provided with a wooden handle. The thermometer stands in a perforated tube on the opposite side of the calorimeter. The cover of the calorimeter is provided with three holes, one for the stem of the thermometer, one in the centre for the paddle, and the third for the handle of the pulveriser. The vessel is supported on three sharp boxwood points within a covered brass case silvered inside, the space between the calorimeter and the case being about 1 cm. all round.

The bath employed for maintaining a constant temperature is practically identical with that described by Nicol,* but is provided with a well in the middle, into which fits closely the outer case of the calorimeter. The temperature of the water or oil is remarkably constant, and when the temperature does not exceed 30° a thermometer in different parts of the bath does not vary by more than ·05°. To assist in maintaining this constancy the bath is covered closely, and is enveloped in thick felt. Three thermometers were used—A, from 8° to 26° C., divided into twentieths; B, from 25° to 50°, and C, from 42° to 76°, both divided into tenths Centigrade, and the temperature could easily be read to ·005° and ·01° respectively, or even half this.

The course of an experiment is as follows:—The water having been introduced into the calorimeter, and the thermometer inserted into its tube, the whole is placed in an oven until the thermometer indicates a temperature about half a degree above that to which the bath has been previously heated. The calorimeter is then put into the bath, the cover of the case is put on, and the thermometer read from time to time till the temperature is either quite stationary or is falling steadily at a rate which is recorded, and from which a correction can be intro-

* "Phil. Mag.," 1883, 339.

duced into the observed result. The glass bulb is then broken, and the paddle gently worked by one person whilst another reads the thermometer by the telescope. In every case a definite interval (usually one or two minutes) is allowed to elapse before the final reading is taken, not only to allow time for solution, but for the alteration of temperature in the liquid to be imparted to the calorimeter. In those cases in which the calorimeter had been in the bath for some time at a rather high temperature, and there was reason to fear that slight loss had occurred by evaporation, the whole was weighed after the experiment was concluded, and a correction introduced accordingly. The amount thus lost never exceeded a gram or thereabouts, and as the thermal equivalent of the calorimeter and its contents amounted to 100 grams and upwards, the result is not appreciably affected.

In the accompanying tables the numbers contained in the column headed "Water equivalent of the calorimeter," include the values of the calorimeter, the glass bulb, the thermometer, and the solution, the specific heat of which is taken generally from Thomsen's numbers.

Three series of experiments were undertaken with sulphate of sodium, and the results are embodied in the three tables, A, B, and C.

For A the salt had been heated to redness, not to fusion, about a fortnight before.

For B the salt had been heated to redness one day before.

For C the precipitated salt had been dried at 120°.

These varieties were taken on account of the differences in the heat of dissolution of sulphate of sodium which have been observed by Mr. Pickering (*loc. cit.*).

Sodium Sulphate, Na_2SO_4, in 100 molecules of Water, or 142 parts in 1800 parts.

Weight of Salt used .. .. 6·311 grams.
Weight of Water used .. 80·000 „

Specific heat of Solution $Na_2SO_4100H_2O$=·927.

Number of expt.	Water equiv. calorim., &c.	Temperature.		Correction.	Total rise.	Molecular heat of dissolution.
		Before solution.	After solution.			
			SERIES A.			
1	110·7	33·46	34·00	+ ·006	·546	1360
2	110·7	33·30	33·86	+ ·010	·570	1419
3	110·6	33·24	33·79	+ ·005	·555	1381
4	110·7	34·97	35·55	0	·580	1444
5	110·6	34·90	35·49	0	·590	1468
6	110·6	35·01	35·58	0	·570	1418
7	110·7	35·41	35·93	+ ·010	·530	1320
8	110·7	39·70	40·30	0	·600	1494
9	110·7	39·60	40·20	0	·600	1494
10	110·7	44·07	44·68	− ·010	·600	1494
			SERIES B.			
11	110·7	31·19	31·70	0	·510	1270
12	110·6	31·50	32·01	0	·510	1269
13	110·7	44·28	44·98	− ·016	·684	1703
14	110·7	44·755	45·490	0	·735	1830
15	110·7	44·57	45·20	− ·01	·620	1544
16	110·7	45·01	45·71	+ ·009	·709	1766
17	110·6	63·51	64·30	+ ·022	·812	2020
18	110·7	67·80	68·69	0	·890	2216
			SERIES C.			
19	110·7	31·60	32·06	0	·460	1146
20	110·6	31·54	32·00	0	·460	1146
21	110·7	45·00	45·55	0	·550	1369
22	110·8	44·98	45·50	+ ·006	·526	1311
23	110·7	60·10	60·84	+ ·01	·750	1869
24	110·7	64·20	64·94	+ ·01	·750	1869
25	110·6	64·20	64·96	+ ·052	·812	2020

Mean Results.

No. in table.	Temp. approx.	Heat of dissolution.		
		A.	B.	C.
11, 12	31°	..	1270	
19, 20	32	..	..	1146
1, 2, 3	33	1387		
4, 5, 6, 7	35	1412		
8, 9	40	1494		
10A, 13B	44	1494	1703	
14, 15, 16B, 21, 22C	45	..	1713	1340
23	60	..	..	1869
17B, 24, 25C	64	..	2020	1945
18	68	..	2216	

From these figures it appears that although the manner in which the salt has been prepared influences the result very notably, yet in each series, comparing together the effects of dissolving the same kind of anhydrous sodium sulphate, the heat of dissolution increases progressively with rise of temperature. That this would be the case might be predicted by applying the principle originally indicated by Person,* and since discussed by Berthelot† and by Thomsen,‡ and embodied in the general equation $Q_T = Q_t + U - V$, where Q_T and Q_t are the quantities of heat evolved in the act of solution at the temperatures T° and t°, and U represents the sum of the capacities for heat of the salt and the water, and V represents the capacity of the solution between the temperatures T° and t°.

The salt A must be regarded as being in the most stable condition of the three, and the following comparison of the numerical values of the thermal change observed and calculated, applies to the A series of experiments:—

$U = (18n + C)(T - t)$.

n is the number of water molecules.

C is the molecular heat of the salt.

$V = (18n + K)(T - t)$.

$C = 32{\cdot}2$ (Kopp).

$18n + K = 1815$ (Marignac).

Take Q_t at $34° = 1400$, which is the mean of numbers given above for 33° and 35°,

* "Ann. Chim. Phys." [3], xxxiii, 449.

† "Ann. Chim. Phys." [4], vi, 329.

‡ "Thermochemische Untersuchungen," I.

Then $Q_T = 1503$ when $T = 40^\circ$.
$= 1537$ when $T = 42^\circ$.
$= 1572$ when $T = 44^\circ$.

From 40° to 44° observed 1494.

There is not much difference here, but the observed is less than the calculated number.

Carbonate of sodium was next examined on account of its close resemblance to sulphate of sodium in water of crystallisation, melting point, and solubility.

Sodium Carbonate, Na_2CO_3, in 100 molecules of Water, or 106 parts in 1800 parts by weight.

Weight of Salt used .. .. 5·005 grams.
Weight of Water used.. .. 85·0 „

Specific Heat of Solution $Na_2CO_3.100H_2O = \cdot 933$.

Number of expt.	Water equiv. calorim., &c.	Temperature.		Correction.	Total rise.	Molecular heat of dissolution.
		Before solution.	After solution.			
26	116·2	21·85	24·37	+ ·075	2·595	6388
27	116·3	21·80	24·25	+ ·058	2·508	6177
28	117·5	22·545	25·050	+ ·1075	2·6125	6485
29	117·4	22·39	24·82	+ ·079	2·509	6238
30	116·2	35·60	38·40	0	2·800	6887
31	116·2	35·28	38·08	0	2·800	6887
32	116·2	35·70	38·50	0	2·800	6887
33	116·3	38·18	40·90	+ ·004	2·724	6708
34	116·3	38·97	41·60	+ ·112	2·742	6753
35	116·1	43·55	46·30	+ ·015	2·765	6785
36	116·3	50·40	53·16	+ ·020	2·780	6846
37	115·1	55·25	58·08	0	2·790	6783
38	114·9	54·73	57·58	0	2·850	6923
39	116·1	54·60	57·50	− ·002	2·898	7128
40	116·2	54·30	57·18	0	2·880	7067
41	115·1	56·00	58·875	0	2·875	7004

The carbonate of sodium used in these experiments had been heated to redness, but at times which varied from a few hours to several days before the experiment. Some of the irregularities in the numbers may be not improbably due to this circumstance. But the mean results show on the whole an increase in the heat of dissolution with rise of temperature.

Experiment number.		Approximate temperature.		Mean heat of dissolution.
26 to 29	......	22°	......	6322
30 „ 33	......	35—40°	......	6842
34 „ 35	......	40—45°	......	6769
36 „ 41	......	50—55°	......	6958

Calculated from the figure for 22° = 6322, taking C = 26·1 (Kopp), $18n + K = 1778$ (Thomsen), T = 55°.

$Q_T = 7909$, which is much higher than 7128, the greatest observed value at 55—56°.

For the sake of comparison sulphate of potassium, which always crystallises without water, was taken with the following results:—

Potassium Sulphate, K_2SO_4, in 100 molecules of Water, or 174 parts in 1800 parts by weight.

Weight of Salt used ..	..	7·733 grams.
Weight of Water used..	..	80·00 „

Specific Heat of Solution $K_2SO_4.100H_2O = {\cdot}8965$.

Number of expt.	Water equiv. calorim., &c.	Temperature.		Correction.	Total fall.	Molecular heat of dissolution.
		Before solution.	After solution.			
43	110·4	15·23	13·015	− ·004	2·211	5494
44	110·3	15·02	13·02	+ ·03	2·03	5037
45	110·4	15·105	13·050	+ ·100	2·155	5348
46	110·3	15·70	13·52	+ ·03	2·21	5472
47	109·4	23·665	21·560	− ·006	2·099	5166
48	109·4	23·90	21·78	0	2·12	5219
49	109·3	36·86	34·93	− ·006	1·924	4720
50	109·4	37·60	35·53	− ·01	2·06	5073
51	109·3	36·99	35·00	− ·008	1·982	4865
52	109·3	45·00	43·13	+ ·075	1·945	4782
53	109·3	45·06	43·20	+ ·046	1·906	4678

Approximate temperature.		Mean heat of dissolution.
15°	....	−5338
24	....	−5192
37	....	−4886
45	....	−4730

Take Q_t at 15° = −5338.

C = 34·1 (Kopp) and $18n + K = 1781$ (Marignac).

T = 45°.

$Q_T = -3745$. Observed −4730. Difference, 985.

In the solution of anhydrous carbonate and sulphate of sodium in water, evolution of heat occurs, and the amount of heat evolved increases with the temperature, but is not so great as would be inferred from the application of Person's formula. On the other hand, sulphate of potassium, the solution of which is attended by absorption of heat, gives a gradually diminishing absorption as the temperature is raised, but the observed absorption is greater at the higher temperature than the calculated amount. That is to say, in all these cases, whether the act of solution is attended by rise or fall of temperature, there is a consumption of energy corresponding to an absorption of heat, which is not accounted for by the difference between the specific heats of the materials, and of the solution which results from their union.

From the connexion already established between fusibility and solubility,* and from *à priori* considerations, it seemed probable that the thermal change attending the solution of a solid must vary according as by raising the temperature the cohesion of the solid is diminished, or by lowering the temperature the cohesion is increased. For if a salt dissolves in water with absorption of heat, this absorption will be less at higher temperatures when the cohesion of the solid has been reduced, and therefore the energy required for its liquefaction by the solvent also lessened. On the other hand, if a salt dissolves with evolution of heat, the heat evolved must be greater at higher temperatures for the same reason.

Some very fusible salts were therefore taken in order to ascertain whether the difference between the observed and calculated heats of solution was notably greater or less than the difference observed in the case of the sodium carbonate and sulphate and potassium sulphates.

* Carnelley, "Phil. Mag.," March, 1882 Tilden; and Shenstone, "Phil. Trans.," 1884, I; Tilden, "J. Chem. Soc.," July, 1884.

Potassium Nitrate, KNO_3, in 100 molecules of Water, or 101·1 parts in 1800 parts by weight.

Weight of Salt used .. .. 4·774 grams.
Weight of Water used .. .. 85 ,,

Specific Heat of Solution $KNO_3 100H_2O$= ·942.

Number of expt.	Water equiv. calorim., &c.	Temperature.		Correction.	Total fall.	Molecular heat of dissolution.
		Before solution.	After solution.			
54	116·8	15·55	12·30	0	3·25	8028
55	116·7	15·50	12·30	0	3·20	7906
56	116·7	34·42	31·30	0	3·12	7703
57	116·7	34·61	31·39	− ·006	3·214	7926
58	116·7	52·72	49·60	− ·015	3·105	7662
59	116·7	53·88	50·87	− ·004	3·006	7421

Mean.

Approximate temperature.		Heat of dissolution.
15°	....	−7967
34	....	−7814
53	....	−7541

From the values C=23·2 (Kopp), $18n+K=1791$ (Thomsen), and $Q_t=-7967$ at 15°, $Q_T=-6751$ when T=53°.

Sodium Nitrate, $NaNO_3$, in 100 molecules of Water, or 85 parts in 1830 parts by weight.

Weight of Salt used .. .. 4·013 grams.
Weight of Water used .. .. 85·0 ,,

Specific Heat of Solution $NaNO_3.100H_2O$= ·950.

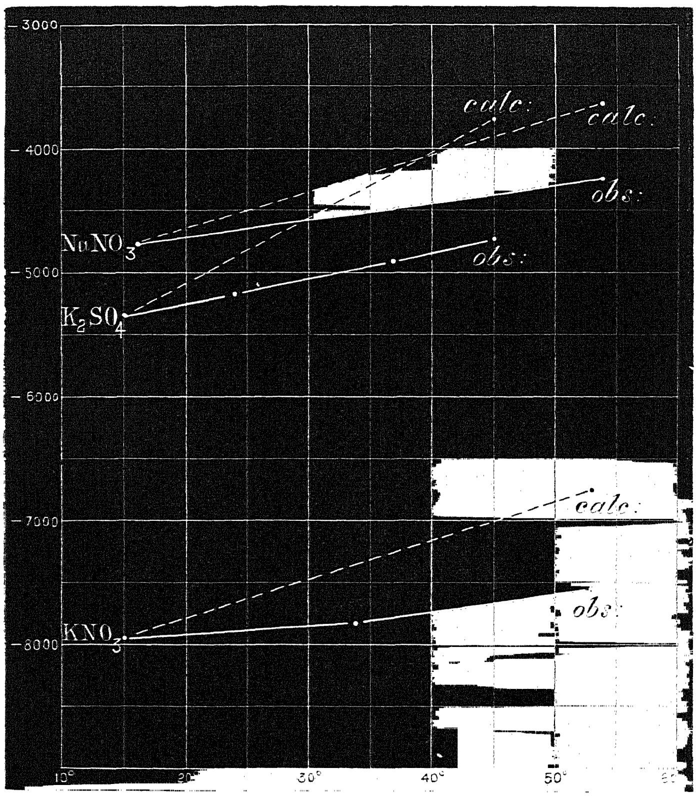
Number of expt.	Water equiv. calorim., &c.	Temperature.		Correction.	Total fall.	Molecular heat of dissolution.
		Before solution.	After solution.			
60	116·8	16·30	14·36	− ·003	1·937	4776
61	116·8	16·05	14·15	+ ·040	1·940	4796
62	116·8	54·42	52·67	− ·004	1·746	4306
63	116·8	54·80	53·09	− ·002	1·708	4204

Mean.

Temperature.		Heat of dissolution.
16°	....	−4786
54	....	−4255

Calculated from the values $C=21{\cdot}8$ (Kopp), $18nK=1791$ (Thomsen), and taking $Q_t=-4786$ at 16°, $Q_T=-3616$ when T is 54°.

The observed and calculated numbers for potassium sulphate and nitrate and sodium nitrate are plotted out in the diagram below, where it is manifest that there is a difference between the two nitrates and the much less fusible and soluble sulphate. For tracing up the abscissa for 45°, we see that the difference between the observed and calculated numbers for potassium sulphate amounts to upwards of 900 calories, whilst the corresponding difference in nitrate of sodium is only about 500, and in nitrate of potassium about 700.



Experiments published in 1873 by Winkelmann,* on the solution of the potassium, sodium, and ammonium chlorides and nitrates in water at different temperatures lead to similar conclusions. Thus taking Winkelmann's figure representing the heat of dissolution of nitrate of sodium, 1 gram of salt in 20·80 grams of water, which is very nearly 1 mol. of salt to 100 mols. of water—

At 2—3° gives as a mean −60·30 cal.
At 50—51° „ „ „ −48·70 „

For a molecule of salt, or 85 grams—

$$60{\cdot}3 \times 85 = 5125{\cdot}5$$
$$48{\cdot}7 \times 85 = 4139{\cdot}5.$$

By the formula—

$$Q_T = Q_t + [(18n + C) - (18n + K)](T - t),$$

and using the values already given for sodium nitrate,

$$Q_T = -3646{\cdot}6,$$

and this is very nearly identical with the number calculated from my own experiment at 16°.

I have made a few determinations of heat of solution of the crystallised sulphate and carbonate of sodium. Whilst these salts dissolve at common temperatures, with great absorption of heat, the absorption at the melting point is very feeble, and at a few degrees higher is changed into evolution of heat. This last-named effect is probably due to the fact that when heated beyond their melting point, 34° (*circa*), these salts are partly deposited in a solid dehydrated state.

* "Pogg. Annalen," cxlix, 1. Winkelmann s experiments were directed towards the object of determining the specific heat of the solutions of these six salts. One difficulty in the way of the general application of this method is the uncertainty attaching to the specific heats of the salts in the solid state, by reason of the variability of their properties according to the manner in which they have been prepared.

Sodium Sulphate, $Na_2SO_4 10H_2O$ in 190 molecules of Water, or 322 parts by weight in 3420 parts of Water.

Weight of Salt used .. .. 8·05 grams.
Weight of Water used .. .. 85·5

Specific Heat of Solution $Na_2SO_4.200H_2O$ = ·955.

Water equiv. calorim., &c.	Temperature.		Correction.	Total change.	Molecular heat of dissolution.	Mean.
	Before solution.	After solution.				
121·4	16·390	12·945	+ ·255	−3·700	−17939	−18035*
121·1	16·250	12·800	+ ·300	−3·750	−18131	
121·4	34·180	34·040	− ·004	−0·136	− 660	− 660
121·3	48·150	48·200	− ·004	+0·046	+ 223	+ 223

Sodium Carbonate, $Na_2CO_3.10H_2O$, in 180 molecules, or 286 parts in 3240 parts by weight of Water.

Weight of Salt used .. .. 7·150 grams.
Weight of Water used .. .. 81·00 ,,

Specific Heat of Solution $NaCO_3.190H_2O$ = ·950 approximately.

Water equiv. calorim., &c.	Temperature.		Correction.	Total change.	Molecular heat of dissolution.	Mean.
	Before solution.	After solution.				
117·3	15·025	12·020	+ ·1312	−3·1362	−14714	−14714
116·1	34·820	34·500	− ·014	·306	− 1405	− 1288
116·1	34·300	34·050	+ ·005	·255	− 1171	
116·1	39·390	39·300	+ ·0075	·0975	− 446	− 446
116·1	47·710	47·690	0	·020	− 91	− 91
116·1	48·040	48·100	0	+0·062	+ 275	+ 311
116·1	48·230	48·300	− ·005	·075	+ 347	

The very rapid decline in the heat absorption attending the solution of these two salts cannot be accounted for by Person's principle, whatever value be considered probable for the specific heat of the solid salt.

The process of dissolving a solid in a liquid must be considered as involving several distinct operations. Person seems to have been the first to point out that the change of state from solid to liquid must

* Thomsen's figure at 18° is −18509.

be distinguished from the intermixture of the liquefied solid with the water. Both these acts must be attended by heat-absorption. The dilution of watery solutions has been shown, especially by the researches of Thomsen, to be attended very generally by absorption of heat, notwithstanding that in most cases it is accompanied by contraction of volume, a process which must have an opposite effect.

In the solution of many anhydrous and even hydrated salts, we must believe, from the very energetic thermal change observed, that the salt enters into chemical union with a portion of the liquid. The extent to which this combination occurs will be greatly influenced by the temperature at which the experiment is made, and at sufficiently elevated temperatures it must be believed to be entirely annulled, whilst on the other hand, as the temperature is raised, chemical action, resulting in double decomposition between the water and constituents of the salt, becomes apparent. This action, which is manifested in the case of the salts of the heavy metals by the production of insoluble oxides, hydroxides, or basic salts, cannot be supposed to be altogether without effect in the case of the salts of the alkali metals such as I have been examining, and I incline strongly to the belief that a part of the difference which I have pointed out between the observed heat of solution and the same calculated according to Person's formula, is due to this decomposing action of the water.

It is manifest that no theory of solution can be accepted which does not take cognisance of all these facts.

In conclusion, I desire to acknowledge the intelligent assistance I have received in the conduct of these experiments from Mr. Harold P. White.

V. "On Radiant Matter Spectroscopy. Part II. Samarium." By WILLIAM CROOKES, F.R.S. Received May 21, 1885.

(Abstract.)

In the concluding sentence of the Bakerian Lecture which I had the honour to deliver before the Royal Society, May 31st, 1883, I said that the new method of Radiant Matter Spectroscopy there described had not only given me spectrum indications of the presence of yttrium as an almost invariable, though very minute, constituent of a large number of minerals, but had likewise revealed signs of another spectrum-yielding element. I stated that I had repeatedly seen indications of another very beautiful spectrum characterised by a strong red and a double orange band.

Elimination of Mercury Vapour from Vacuum Tubes.

It is much more difficult than is generally supposed to keep mercury vapour from diffusing into the experimental tubes.

The following plan answers perfectly so far as my experiments have yet gone:—Sulphur is first prepared by keeping it fused at a high temperature till bubbles cease to come off, so as to get rid of water and hydrogen compounds. It is then allowed to cool, and is pounded and sifted so as to get it in the form of granules averaging a millimetre in diameter. A glass tube, a centimetre in diameter and about 2 feet long, is lightly packed for half its length with this sulphur, and next about 2 inches of iodide of sulphur (I_2S_2) is added, and the rest of the tube is then filled up with sulphur. Ignited asbestos is packed in at each end to keep the sulphur from blowing out whilst the vacuum is being made, or from being sucked through when air is suddenly let in. This contrivance entirely keeps mercury vapour from passing through, since the iodide of sulphur holds its iodine very loosely, and fixes the mercury in the form of non-volatile red iodide. A glass tube containing finely divided copper must follow in order to keep sulphur out. With this blockade interposed between the pump and experimental tubes I have been unable to detect mercury vapour in any of the tubes, whether in the cold or on heating them.

The "Orange Band" Spectrum.

Since the date of my last paper I have devoted myself to the task of solving the problem presented by the double orange band first observed in 1881. With the yttrium experience as a guide it might be thought that this would not be a difficult task, but in truth it helped me little beyond increasing my confidence that the new, like the old, spectrum was characteristic of an element. The extreme sensitiveness of the test is a drawback rather than a help. To the inexperienced eye one part of "orange band" substance in ten thousand gives as good an indication as one part in ten, and by far the greater part of the chemical work undertaken in the hunt for the spectrum-forming element, has been performed upon material which later knowledge shows does not contain sufficient to respond to any known chemical test.

Chemistry, except in few instances, as water-analysis and the detection of poisons, where necessity has stimulated minute research, takes little account of "traces;" and when an analysis adds up to 99·99, the odd 0·01 per cent. is conveniently put down to "impurities," "loss," or "errors of analysis." When, however, the 99·99 per cent. constitutes the impurity and this exiguous 0·01 is the precious material to be extracted, and when, moreover, its chemistry is

absolutely unknown, the difficulties of the problem become enormously enhanced. Insolubility as ordinarily understood is a fiction, and separation by precipitants is nearly impossible. A new chemistry has to be slowly built up, taking for data uncertain and deceptive indications, marred by the interfering power of mass in withdrawing soluble salts from a solution, and by the solubility of nearly all precipitates in water or in ammoniacal salts, when present in traces only. What is here meant by "traces" will be better understood if I give an instance. After six months' work I obtained the earth didymia in a state which most chemists would call absolutely pure, for it contained probably not more than one part of impurity in five hundred thousand parts of didymia. But this one part in half a million profoundly altered the character of didymia from a radiant-matter-spectroscopic point of view, and the persistence of this very minute quantity of interfering impurity entailed another six months' extra labour to eliminate these final "traces" and to ascertain the real reaction of didymia pure and simple.

Chemistry of the Orange Band-forming Substance.

At first it was necessary to take stock, as it were, of all the facts regarding the supposed new substance, provisionally termed x, which had turned up during the search for the orange band. In the first place x is almost as widely distributed as yttria, frequently occurring with the latter earth. It is almost certainly one of the earthy metals, as it occurs in the insoluble oxalates, in the insoluble double sulphates, and in the precipitate with ammonia. It is not precipitated by sodic thiosulphate, and moreover it must be present in very minute quantities, since the ammonia precipitate is always extremely small, and as a rule x is not found in the filtrate from this precipitate.

At this stage of the inquiry the chemical reactions of x were much more puzzling than with yttria. At the outset an anomaly presented itself. The orange band was prone to vanish in a puzzling manner. Frequently an accumulation of precipitates tolerably rich in x was worked up for purposes of concentration, when the spectrum reaction suddenly disappeared, showing itself neither in precipitate or filtrate; whilst on other occasions, when following apparently the same procedure, the orange band became intensified and concentrated with no apparent loss. The behaviour of the sulphate to water was also very contradictory; on some occasions it appeared to be almost insoluble, whilst occasionally it dissolved in water readily.

Is "x" a Mixture?

A very large series of experiments, which need not here be described in detail, resulted ultimately in establishing the remarkable fact that the x I sought was an earth which of itself could give no

phosphorescent spectrum in the radiant matter tube, but became immediately endowed with this property by admixture with some other substance, which substance likewise by itself had no power of phosphorescing with a discontinuous spectrum.

"x" *in Cerite.*

In the corresponding yttrium research I was aided materially by the fact that the sought-for earth did not give an absorption spectrum. This enabled me to throw out a large number of obscurely known elements, and I therefore early endeavoured to ascertain whether the supposed new earth, x, did or did not give an absorption spectrum. Gradually it was noticed that whenever the didymium absorption bands were strong the orange band spectrum was also particularly brilliant. Moreover, amongst the earths previously enumerated as mixed with lime in the quest for x, I have mentioned that some of them gave the orange band spectrum with increased intensity; the earths of the cerium group were the most noteworthy, and these considerations made it probable that here would be found the location of x.

Analysis of Cerite.

The cerium group consists of cerium, lanthanum, didymium, and samarium.

The first necessity was to get the earths ceria, lanthana, and the mixture hitherto called didymia, in a pure state, for my so-called pure earths of this group all showed the orange band in more or less degree.

The separation from each other of ceria, lanthana, didymia, and samaria is a most laborious process, and the amounts of these earths, obtainable in anything like a pure state, is small, compared with the mass of material worked up. Full particulars are given in the paper as to the method adopted to obtain each of them in a state of purity.

Ceria.

The ceric oxide obtained was almost pure white. A considerable thickness of a strong solution did not show a trace of absorption spectrum. The atomic weight of the metal was taken and yielded the number 141·1.

The ceric oxide gave no orange band spectrum in the radiant matter tube, either with or without the addition of lime.

Lanthana.

Lanthana is more difficult to purify than ceria. Long after the lanthana appeared pure, it gave in the radiant matter tube a good

orange band spectrum when mixed with lime and treated as usual, although without lime it gave no spectrum. Ultimately, however, a lanthana was obtained which, mixed with lime and treated in the usual manner, gave no orange band spectrum whatever. This lanthana was snow-white, and had an atomic weight of 138·3.

Didymia.

The earth formerly called didymia is now known to be a mixture of didymia and samaria, and systematic operations were now commenced with the object of obtaining the didymia and the samaria in a state of purity—that is to say, in such a condition that one of them should show no orange band spectrum at all, whilst the other should give the spectrum in its highest degree of intensity.

I commenced the purification of didymia in the latter part of the year 1883, and the operations have been going on since almost daily in my laboratory. At intervals of some weeks the didymia in the then stage of purification was tested in the radiant matter tube, a little lime having previously been added to bring out the discontinuous phosphorescence. During the first month or two the intensity of the orange band spectrum scarcely diminished. After this it began to fade, but the last traces of orange band were very stubborn, and not till the last few weeks could I obtain a didymia to show no trace of the orange band spectrum; and this result has not been accomplished without sacrifice. My 1000 grammes have dwindled away bit by bit, till now less than half a gramme represents all my store.

Samaria.

The foregoing experiments left little doubt that x, the orange-band-forming body, was samarium; the last problem was, therefore, to get this earth in a pure state. The general plan of operations was the same as I adopted in getting didymium free from samarium, only attention was now directed to the portions richest in samarium which had been formerly set aside. The colour of samaria, as pure as I have been able to prepare it, is white with the faintest possible tinge of yellow. The absorption spectrum of samarium salts is much more feeble than the spectrum of didymium.

The Phosphorescent Spectrum of Samarium.

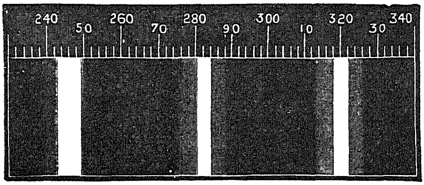
Pure samaric sulphate by itself gives a very feeble phosphorescent spectrum. When, however, the samaria is mixed with lime before examination in the radiant matter tube, the spectrum is, if anything, more beautiful than that of yttrium. The bands are not so numerous, but the contrasts are sharper. Examined with a somewhat broad slit, and disregarding the fainter bands, which require care to bring them

out, the spectrum is seen to consist of three bright bands,—red, orange, and green,—nearly equidistant, the orange being the brightest. With a narrower slit the orange and green bands are seen to be double, and on closer examination faint wings are seen, like shadows to the orange and green bands.

Preliminary experiments had shown me that lime was one of the best materials to mix with samaria in order to bring out its phosphorescent spectrum, but it was by no means the only body which would have the desired effect.

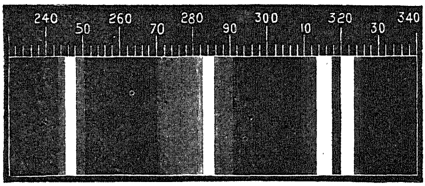
The samarium spectra, modified by other metals, may be divided into three groups. The first group comprises the spectra given when glucinum, magnesium, zinc, cadmium, lanthanum, bismuth, or antimony is mixed with the samarium. It consists simply of three coloured bands, red, orange, and green; as a typical illustration I will select the lanthanum-samarium spectrum (fig. 1).

Fig. 1.



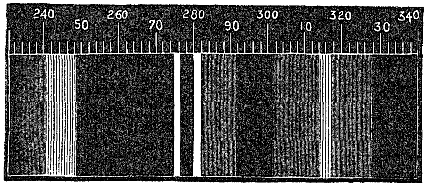
The second type of spectrum gives a single red and orange and a double green band. This is produced when barium, strontium, thorium, or lead, is mixed with samarium. The lead-samarium spectrum (fig. 2) illustrates this type.

Fig. 2.



The third kind of spectrum is given by calcium mixed with samarium. Here the red and green are single, and the orange double. Aluminium would also fall into this class were it not that the broad ill-defined green band is also doubled. The calcium-samarium spectrum (fig. 3) is a good illustration of this type.

FIG. 3.

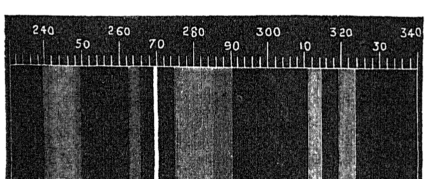


Mixed Samarium and Yttrium Spectra.

It was interesting to ascertain what spectrum a mixture of samarium and yttrium would give. A mixture of 90 parts of samaria to 10 of yttria was treated with sulphuric acid and then ignited, and afterwards examined in the radiant matter tube. The result was as remarkable as it was unexpected. Not a trace of the yttrium spectrum could be detected. The powder phosphoresced with moderate intensity, but the spectrum was almost the facsimile of that given by pure samaric sulphate, except that the sharp orange line, which in the spectrum of pure samaric sulphate is only just visible, had gained sufficiently in intensity to be measurable, and was found to lie at 2693, on the $\frac{1}{\lambda^2}$ scale. A large number of experiments were next tried on mixtures of samaria and yttria in different proportions, and the results are given in full in the paper.

Up to mixtures of 43 parts samaria and 57 parts yttria the spectrum nearly resembled the lead-samarium spectrum. Not a band of the yttria spectrum could be detected, and the brilliant orange line stood out sharply in the whole series. This spectrum is represented in fig. 4.

Fig. 4.



After that proportion had been reached a change rapidly came over the spectra, and in the next trial mixture—samaria 35 yttria 65—the only indication of the samarium spectrum that could now be found was seen in the two faint green bands next to the citron line of yttria, and the new orange line, which shone out as brightly and sharply as ever.

It will be remarked that a sudden change of spectrum occurs between very narrow limits of mixture.

The spectrum of a mixture of 44 parts samaria and 56 parts yttria, except for the orange line, is the pure samarium spectrum. The spectrum of 42 samaria and 58 yttria is built up of some of the component bands of the spectrum of each earth; whilst the spectrum of 39 samaria and 61 yttria is almost a pure yttria spectrum, the sharp orange line running across them all.

The Delicacy of the Spectrum Test for Samarium.

Experiments were now commenced with the object of getting some approach to a quantitative estimate of how small a quantity of samarium could be detected.

A mixture was first made in the proportion of one part samarium to 100 parts of calcium. The spectrum is very brilliant, and but little inferior in sharpness to the spectrum given by a 50 per cent. mixture.

A mixture was now prepared containing 1 part of samarium to 1000 parts of calcium. Very little difference can be detected between the spectrum of this mixture and that of the last. The bands are, however, a little less sharp.

A mixture containing one part of samarium to 10,000 parts of calcium was now tested. The bands are now getting fainter, the second green band is fading out, and the continuous spectrum of calcic sulphate is getting brighter.

The next mixture tried contained one part of samarium in 100,000 parts of calcium. Here the green is almost gone, being overshadowed by the continuous spectrum of calcium which has spread over it. The red band has likewise almost disappeared in the greater brightness of the continuous red of the calcic spectrum. The double orange band is still very prominent, and the black space, 2942, between it and the green is very marked.

The next mixture, one part of samarium to 500,000 parts of calcium, gives a spectrum which is fainter than the last, but the orange bands are still distinctly visible. The blank space between the yellow and green is strongly marked, but narrower than before.

A mixture of one part of samarium in 1,000,000 parts of calcium was next subjected to experiment. In this the samarium spectrum is very feeble, and the orange bands are only to be seen with difficulty. Now the most striking characteristic of this spectrum is the black space which still cuts out the greater portion of the yellow.

A mixture of one of samarium in 2,500,000 parts of calcium was now taken. In the spectrum shown by this mixture the bands of samarium have entirely gone, and its presence now is apparent only

by the darkening in the yellow portion of what otherwise would be a continuous spectrum.

The calcium phosphorescent spectrum by itself is continuous, with no break, lines, or bands in it.

The Anomalous Line $\frac{1}{\lambda^2}$ 2693.

On several occasions I have spoken of an orange line, 2693, which by its brilliancy and sharpness is a prominent object in most of the samarium-yttrium spectra. With pure samaric sulphate it is exceeding faint. With samaria containing 5 per cent. of yttria it is very little brighter; with 10 per cent. of yttria it gains a little; with 15 per cent. it is brighter still, and with a mixture of 80 parts samaria and 20 parts yttria it is at its maximum intensity. It continues to be the most striking feature in the spectra of the various mixtures of samaria and yttria until the proportion becomes samaria 3, yttria 97, when it begins to get less bright, and only when pure yttria is reached does it altogether vanish.

It is noteworthy that so long as this bright line is a component of the spectrum, the other bands manifest decidedly less intensity, and many of them are suppressed. The profound modification in the spectra of samaria and yttria developed by their mixture is, I believe, without precedent in spectrum analysis. It is difficult to realise the character of the modification which converts somewhat faint diffused bands into one intensely sharp and brilliant line.

One important lesson taught by the many anomalies unearthed in these researches is, that inferences drawn from spectrum analysis *per se* are liable to grave doubt, unless at every step the spectroscopist goes hand in hand with the chemist. Spectroscopy may give valuable indications, but chemistry must after all be the court of final appeal.

VI. "On the Microscopic Characters of some Specimens of Devitrified Glass; with Notes on certain analogous Structures in Rocks." By DOUGLAS HERMAN and FRANK RUTLEY. Communicated by Professor T. G. BONNEY, D.Sc., F.R.S. Received May 28, 1885.

(Abstract.)

This paper relates first to the changes brought about in glass solids bounded by plane surfaces, by exposure to high temperatures, the main object of the paper being to elucidate the changes which have taken place in vitreous, and once vitreous, rocks by comparing the phenomena of natural devitrification with similar phenomena effected

by artificial means. The first case considered is that of a piece of thick plate glass which has been totally devitrified under conditions described in detail in the paper. In the roughly broken specimen the fractured surface sufficiently reveals the fact that crystallisation has taken place throughout the mass, while other specimens subsequently described by the authors demonstrate beyond question that the crystallisation has started at the surfaces and has travelled inwards. In this particular specimen the crystallisation has advanced from the different surfaces until the various sets of crystals have arrested one another in very definite planes, whose traces are clearly perceptible on a fractured surface. A figure showing this has recently been published in Prof. Bonney's Presidential Address to the Geological Society, the drawing having been made from a chip taken from part of the same plate of glass now in the Museum of Practical Geology. An examination of a section cut in one direction from the specimen now described, showed little more than has already been stated, except that the crystals were seen to occur in bundles of divergent prisms, the centres of divergence being at or near the surfaces of the plate of glass. An inspection of a ground surface of the plate showed a curious reticulation, in some places very well marked, and it therefore seemed desirable to examine another section taken parallel to this surface, and at right angles to the former section. This was done, and it then became apparent that a third section must be cut, so as to traverse two directions of crystallisation disposed at right angles to each other before any sound interpretation of the phenomena could be arrived at. This third section traversed one of the triangular areas, or rather wedges formed by arrest, on the shorter side of the plate, at a little distance from the thin end of the wedge, and, beyond this area, it passed parallel to the adjacent directions of crystallisation. In this section the bundles of crystals lying normal to the plane of section were seen exactly to resemble those visible in the second section taken parallel to one of the broad surfaces of the plate, while the others were identical with those already seen in the first section. It then became clear that a system of prismatic jointing had been developed, presumably due to strain around separate centres of crystallisation from which the divergent fasciculi emanated, while from the first and third sections it was evident that this jointing extended from the surface inwards, until the development of one set of crystalline bundles was arrested by another set. In sections taken parallel to these fasciculi there is a curious wavy brindling, which is apparently due to structure analogous to that seen in "cone in cone" ironstone and some other minerals, the marked character of the phenomena when viewed by transmitted light being probably due to want of coincidence in the direction of the minute prisms or fibres composing the overlapping divergent groups. In order to ascertain more fully the conditions

under which this mode of devitrification would hold good, glass solids of definite geometrical forms were partially and completely devitrified, and it was seen that in each case the crystalline bundles, separated by joint planes, extended from the surface of the solids inwards in directions normal to the surfaces. In a trigonal prism it was found that there was an apparent departure from this rule, certain sets of crystals emanating from lines within the prism, and extending outwards towards the surfaces of the solids, although they did not reach those surfaces. This anomalous procedure was referred to the presence of flaws, and to demonstrate this two fresh specimens were devitrified. One was another trigonal prism free from any flaw, in which devitrification extended uniformly from the surfaces until three straight lines of arrest resulted, meeting in the axis of the prism. The other specimen was a thick piece of flat plate glass. On this a short line was ruled with a diamond, and a tap being administered, a crack was developed from this line, extending rather more than half way through the plate. The plate was then completely devitrified. The result was precisely similar to that seen in the specimen first described, but it was found that the crack emanating from the diamond scratch had acted as a fresh *surface*, devitrification having taken place at right angles to this crack as far as the median line of arrest, while on the other side of that line, and from the extremity of the crack, a radiating crystalline structure was developed, resulting in a structure analogous to that of a hemisphere, but of course extending the whole length of the crack.

The remainder of the paper touches upon the incipient development and formation of spherules and crystallites, with other points of interest in connexion with superficial devitrification. The spherules in their most rudimentary stage sometimes appear to consist of nebular segregations of globulites. Instances are also cited in which partial and complete devitrification of a glass plate is brought about by the development of spherules in the interior of the plate in conjunction with the formation of an enveloping crust, which extends to variable distances from the surfaces of the plate, according to the period of devitrification, and also, more especially, according to the chemical composition of the glass.

Some notes are also given on the relation of the phenomena here described to similar ones met with in naturally devitrified rocks, the paper being intended as the forerunner of others, in which it is hoped that the petrological bearings of the subject may be more fully discussed.

VII. "Regional Metamorphism." By JOSEPH PRESTWICH, M.A., F.R.S., Corr. Acad. Sci. Paris, Professor of Geology in the University of Oxford. Received June 11, 1885.

Metamorphic rocks have been divided into two classes—1. Those in which local changes have been caused by contact with heated eruptive rocks; 2. Those extending over wider areas, in which the rocks are in no apparent relation to eruptive or igneous rocks. The first has been termed *Contact Metamorphism*, and the second *Normal or Regional Metamorphism*, the latter two terms having been used to express the same phenomena and treated as synonymous.

The object of this paper is briefly to show that there may be another cause for metamorphic action, for which, not to introduce a new term, I would propose to transfer and restrict the term of "*Regional Metamorphism.*" Normal metamorphism I would confine to signify, as hitherto, the changes caused by the heat due to depth, on the supposition of the existence of a heated central nucleus of the earth, while I would use the term *regional metamorphism* to denote changes effected by the agency of the physical causes to which Mr. Mallet referred the fusion of the volcanic rocks, namely, *the heat produced locally within the crust of the earth by transformation into heat of the mechanical work of compression, or of crushing of portions of that crust.**

I was led to consider the importance of this action by the abnormal result presented in the distribution of the underground isotherms in the St. Gothard Tunnel, and which on looking into the question can only, as it seems to me, be attributed to the residual heat arising from the crushing of the rocks during the upheaval of that portion of the Alpine range, which is of very late geological date; and also by some cases in which the alteration in the rocks hardly seemed explicable upon the hypothesis either of ordinary contact- or normal-metamorphism.

This other source of heat had not been altogether overlooked by geologists, though only occasionally referred to as a secondary cause; but its actual importance had hardly been realised until Mallet investigated the subject experimentally and mathematically. He failed to show sufficient cause for the fusion of the volcanic rocks, but he drew attention to the enormous heat-producing power of certain earth movements. This power, inadequate though it may be to explain the phenomena of vulcanicity, is singularly applicable in explanation of some of the metamorphic phenomena exhibited in mountain ranges. The object of his experiments, however, having been to establish the maximum results to be attained by the force of compression, only bears indirectly on the collateral problem we are here considering.

The primary object of Mr. Mallet's experiments was to ascertain

* "Phil. Trans. Roy. Soc." for 1873, p. 147.

the force required to crush portions of various rocks of given size, and to determine the quantity of heat evolved by the process. For this purpose, as I have before mentioned,* the work done was measured by the proportion of water at 32° F. that could be converted into steam of one atmosphere (or at 212° F.) by the estimated heat evolved by the crushing of 1 cubic foot of each class of rock.

The crushing weight in the case of the specimens of the Sedimentary Strata was found to vary in round numbers from about 2 to 18 tons per square inch, while for the Crystalline and Igneous rocks it reached to over 30 tons (each class of rock showing considerable variations); and the temperature resulting from crushing 1 cubic foot of rock varied from 8° F. in Caen Oolite to 217° F. in Guernsey Granite. In conclusion Mallet estimated that the heat of liquefaction of 1 cubic mile of ice at 32° melted is equivalent to the crushing work of 1·277 cubic mile of mean rock when transformed into heat, or that 1 cubic mile of crushed mean rock would fuse 0·108 cubic mile of volcanic material.

With all the harder rocks the heat produced in the metal surroundings by the complete crushing was easily perceptible by the hand, and was so great with some of the granites and porphyries as to necessitate a delay for the apparatus to cool. Both Mr. Mallet and Professor Rankine were of opinion that in the crushing of a rigid material such as rock, *almost the entire* mechanical work (with the exception of a small residue of external work) reappears as heat. It was further shown that, even in the most rigid bodies, crushing begins by compression and yielding, and that at this stage heat begins to be evolved.

Mr. Mallet, applying these results to the deformation caused in the earth's crust by the contraction of the cooling nucleus, observes that the compression will be greatest along lines of fault and mountain-upheavals, so that the chief amount of the work of compression will be transferred to those lines of fracture or weakness; and the increase of temperature produced by the greater part of the internal work will cause the parts of the crust about those lines to become much hotter than the intervening parts, where the crust is undisturbed.

Consequently the work thus developed being transformed into heat, that heat will be greatest along those lines or planes at places where the movement and pressure, together constituting the work, is greatest; whence Mallet concluded that along or about such axial lines of concentrated compressive and crushing work, the temperature may locally rise to a red heat, or even to that of fusing the rocky materials crushed and the pressing-together-walls themselves adjacent to them. This was, in his opinion, the real nature and origin of the volcanic heat as now produced on the globe.

* "Proc. Roy. Soc." for May, 1885.

Although the hypothesis fails for various reasons* in its application to vulcanicity, for, as a matter of observation, the great lines of disturbances and compression of the Alps, Pyrenees, and other mountain-chains are free from either active or extinct volcanoes; there is nevertheless reason to believe that this source of heat may have been adequate to produce great molecular changes in the rocks along the lines of disturbance and upheaval. It is precisely along such lines that not only are the older rocks metamorphosed, but rocks of cretaceous and tertiary age—which usually have not been affected by normal metamorphism—coming, in these mountain-chains, under the influence of the disturbing forces, have undergone a change analogous to that produced by normal metamorphism.

[Unfortunately no other experiments than those of Mr. Mallet, to determine the heat developed by the compression of rocks, have been made; and valuable as his experiments are, his full conclusions cannot be accepted, because in nature the complete crushing upon which his calculations are based does not obtain, nor can the heat be localised in the way assumed.

On the other hand, his experiments do not take into separate consideration *friction* and *deformation*, the influence of which in raising the temperature during earth-movements must be very considerable. No special experiments have, in fact, been made on the work of these forces on rocks, but it has been proved experimentally with metals. Iron can be raised by those means to a low red heat, and it has been estimated that with lead the rise of temperature under deformation is equivalent to 700° F.

Further, Mallet's estimates were based on an initial temperature of 32°, whereas the initial temperature in the underground rocks affected by the earth-movements would necessarily be high. Consequently, although we cannot accept the extreme estimates of Mallet—nor can we hope to ascertain with the imperfect data at present in our possession the exact heat developed by rock movements—still it is certain that his experiments, combined with what is known of the heating effects of friction and deformation, indicate that a large amount of heat must be developed in the underground rocks by these causes.—July 6, 1885.]

If the disturbances had taken place at once, or suddenly, and the rocks had been wholly crushed, the results calculated on by Mr. Mallet were more likely to have been attained. But the movements were in all probability of extreme slowness for very long periods—and this might be an argument that they were so—and it was only when the tension had reached a certain point that disruption took place, and the movement of the parts became more rapid, pending the restora-

* For some other of the physical objections, see the Rev. O. Fisher's "Physics of the Earth's Crust," Chapter XVIII.

tion of a state of equilibrium; nor is there any reason to suppose that the rocks were at any time crushed in the complete manner accomplished experimentally by Mr. Mallet. Consequently much of the heat developed would be dissipated during the long slow compression, and the maximum effects estimated by him would not obtain in nature. Still, as the experiments show that the weight at which the first yielding of the rock takes place is not more than one-third of the crushing weight, the thermal effects might still be considerable, provided the time the heat had to spread through the adjacent rocks were not excessive. It is also certain that greater and more concentrated effects would result at the time of actual disruption and faulting. The gigantic foldings of the rocks in the Alps, and the magnitude of the faults there and in the Pyrenees, show how vastly great the forces then in operation were; and indicate how important must have been the consequent rapid conversion of even a portion of the work of these forces into heat.

Amongst the objections that have been raised against the explanation of some cases of alteration of sedimentary strata in mountain-chains by ordinary normal metamorphism, is the one that unaltered strata alternate with the altered strata. Sometimes an apparent alternation is explained by inversion of the strata, or where that does not exist, it may be due to the circumstance that differences of mineral composition, or in the proportion of the water of imbibition, have caused the metamorphism to affect different beds in different degrees. On the theory of *regional metamorphism*, in the sense I would use it, another explanation suggests itself by the way in which differences in the resistance of the rocks develop different quantities of heat. Mr. Mallet has shown by experiments on the compressibility of rocks at Holyhead, that, although certain slate-rocks were compressed by precisely the same force before their elastic limits were passed, yet owing to differences in their compressibility, the heat developed in the rocks when released would render the quartz-rock nearly three times as hot as the slate-rock. In this manner, therefore, it may be possible to account for a special and restricted metamorphism of some strata in mountain-chains, and for its frequently localised occurrence.

Further there are, as is well known, many strata which are not usually metamorphic, but which are so when involved in mountain-chains. Among many common examples of such metamorphism may be instanced that of the lower cretaceous strata on the flanks of the Pyrenees. They are there represented by dark schists and crystalline limestones, while at a short distance from that range they consist of marls and ordinary limestones. In the Alps, strata of middle Eocene age are, at the Diablerets, converted into hard black slaty rocks, which are purely local; while the soft and earthy calcareous Nummulitic

strata of the south of France are represented by massive limestones and crystalline marbles. Normal metamorphism cannot here be invoked, because it does not appear that these rocks have been covered by any great thickness of newer rocks, or depressed to such depths so as to bring them within the influence of the higher underground temperatures. Nor can the change be always attributed to contact metamorphism with granite or other eruptive or protrusive rocks. It is only in cases where there is a central axis of any eruptive rocks that this form of metamorphism can have acted, but in the many cases where no eruptive rocks appear, the effect may, I would suggest, be due entirely to *regional metamorphism.*

The remarkable changes which take place in the condition of the coal of Pennsylvania, as it ranges into the Appalachian Mountains, may also be owing more probably to regional than to normal metamorphism. This mountain-range consists of a series of great parallel folds, increasing in acuteness as the central axis is approached. Eruptive rocks are absent; but nevertheless the strata, as they approach the central chain, become more crystalline, and the coal, which at a distance is ordinary bituminous coal, passes into anthracite, and even graphite. The late Professor H. D. Rogers divided this great coalfield into four basins. The coal in the less disturbed district near the Ohio river, where the flexures are extremely gentle and wide apart, contains from 40 to 50 per cent. of volatile matter; in the wide basin further east it decreases to 30 or 35 per cent.; in the basins of the Alleghany range, in which, although there are no important dislocations or great flexures, there are some extensive and symmetrical anticlinical axes of the flatter form, the proportion of the volatile matter in the coal varies from 16 to 22 per cent.; while in the most easterly chain of basins which are associated with the boldest flexures and greatest dislocations, with close plications and inversions of strata, the quantity of volatile matter in the coal is reduced to from 6 to 14 per cent.*

A somewhat analogous instance is presented by the Carboniferous series of Belgium. The excessive squeezing, faulting, and inversions which the Coal-measures have undergone on the flanks of the axis of the Ardennes, is there accompanied by an alteration of the highly bituminous coals into dry coals and into anthracite; while the Carboniferous and Devonian limestones amidst the sharply convoluted and folded strata of the Ardennes are there, as they are also on the line of the same disturbance in the Boulonnais, transformed very generally into semi-crystalline marbles. The few exposures of eruptive

* Some geologists have referred the coincidence of these phenomena, partly to the greater facility afforded for the escape of volatile matter when the fracturing of the rocks has produced an infinite number of cracks and crevices, and partly to the gases and waters which penetrated these cracks and promoted the disengagement of volatile matter.

rocks are all on a small scale, and affect the adjacent rocks locally only by contact metamorphism. It is probable that the anthracite of the coal-field of South Wales is the result of similar *regional metamorphism*.

In the case of certain contact metamorphisms produced by contact with igneous rocks, we know that the changes were produced by great heat, for many igneous rocks must have had a temperature of 3000° to 4000° F. or more; while in the case of normal metamorphism it is evident that the changes produced did not depend so much on high temperature as on pressure and the presence of water; and there is reason to believe that a temperature of about 600° to 800° F. would suffice to produce all or almost all the observed hydrothermal effects. For although in many instances of normal metamorphism new minerals are formed, the rocks are not fused, nor are the fossils destroyed. In Brittany, black slates, which pass into schists, with large crystals of Chiastolite, still show impressions of *orthis*, *trilobites*, and other Silurian fossils.* Devonian strata in the Vosges pass into a rock consisting of pyroxene, garnet, epidote, and other silicates of this character, and yet retain impressions of *corals*.†

The degree of heat required, therefore, to produce changes similar to those produced by normal metamorphism, under somewhat analogous conditions of time, temperature, and moisture, is comparatively small; and as affording some indication of this amount, the alterations in the character of the coal which have taken place in the above-named instances afford a convenient approximate test. While it requires a red heat to convert coal into coke, its conversion into anthracite is effected in presence of moisture at much lower temperatures, and while contact with igneous rocks has produced the former effects, contact with granite has only resulted in the latter. M. Daubrée has even converted wood, by exposure for some time in water under pressure to a temperature of 300° C., into an anthracite so hard as scarcely to be touched by steel, and so infusible as to burn with extreme slowness even in the oxidising flame of the blowpipe, while at the same time it has been rendered, like the diamond, a non-conductor of electricity. This alteration in the coal-beds indicates, therefore, the influence of a temperature sufficient to produce effects similar to those produced by ordinary normal metamorphism, and consequently sufficient to raise very considerably the temperature of a body of rocks such as form mountain-chains, though insufficient to cause fusion.

* Bobblaye: "Bull. Soc. Géol. de France," vol. x, p. 227. [Mr. John Postlethwaite, of Keswick, informs me that Graptolites and the fragment of a Trilobite have recently been found in the metamorphic "Chiastolite" slate of Skiddaw. —July, 1885.]

† Daubrée: "Géologie Experimentale," p. 141.

Of the enormous tangential pressure exercised in the elevation of these chains, some idea may be formed when we consider the amount of compression which those portions of the crust have undergone. Thus, for example, as I mentioned in the paper before referred to, Heim estimates that in the Alps the compression has been to the extent of 72 miles; and in a recent paper by Professor Claypole, he arrives at the conclusion, after a careful investigation of the magnitude and width of each fold, that in the Appalachian mountains "a tract of the earth's surface measuring originally 153 miles from south-east to north-west has been so crushed and compressed that its present breadth along the line of section is only 65 miles," and of this, in one part,—the Cumberland Valley,—"95 miles of country have been compressed into 16 miles."

These vast compressions could not have taken place without the transformation into heat of a large portion of the mechanical work, though the degree and centralisation of the heat would depend on the rapidity and completeness with which the compression and deformation had been effected. Need we therefore be surprised to find that, in some of the newer mountain-ranges, a small residual portion of the heat thus mechanically evolved still existing and causing slight aberrations in the position of the underground isothermal lines: the same cause may possibly account for other exceptional cases.

The only sufficiently complete set of observations on a mountain-chain of this character that have yet been made are those I have before alluded to, by Dr. Stapff in the St. Gothard Tunnel. Particulars of these observations will be found in my paper on "Underground Temperatures," and I need therefore here only mention that they show at the north end of the tunnel in the part where an axis of elevation of late geological age (Pliocene) traverses the range, that the thermic gradient, which normally equals about 57 feet for 1° F., is there not more than 38 feet; and for this Dr. Stapff states that there was no obvious explanation.

In further support of this view, I would refer to the exceptional frequency of thermal springs in mountain chains. Some of these are no doubt due to the presence of eruptive rocks, but in many cases there are none of these rocks in the neighbourhood, and yet hot springs are common. Others may, of course, be due to the normal temperature of the depth from which the water rises, but their numbers and their position often militate against this view. In the Alps they are not infrequent, and sometimes occur at very high levels. In the Pyrenees the number of thermal springs exceeds 150, while the Professors Rogers ascertained that there were 56 such springs in the Appalachian chain of mountains. Seven of these are on lines of fault or inversion; the others issue on lines of anticlinal axes, or at points near to them.

If I have in these fe remarks shown cause for believing that we

have in the compression and friction of the strata which has always accompanied the upheaval of mountain-chains a *vera causa* for the production of an amount of heat sufficient to produce one form of metamorphic action—a form which can affect only particular regions —I think it would be desirable, in order to show its distinctiveness from either contact or normal metamorphism, to designate it by the term of "Regional Metamorphism." In any case, I trust I have shown cause for further inquiry.

VIII. "On the Mydriasis produced by the local Application of Cocaine to the Eye." By WALTER H. JESSOP. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received June 4, 1885.

In a paper on "Cocaine," published in the "Practitioner" of January 1, 1885, and more fully in a paper on "The Cocainised Eye," before the Ophthalmological Society on January 8th, 1885, I mentioned most of the clinical facts concerning the drug and its action that had then come under my own investigation and treatment.

The object of this paper is to try and elucidate one of these facts, namely:—The cause of the mydriasis accompanying the application of cocaine to the eye. This research has been made chiefly on human and rabbits' eyes by conjunctival instillation, and on rabbits' eyes by experiments detailed below. The salt of cocaine used has been the hydrochlorate obtained from Merck, of Darmstadt; the strength of the solutions 2, 4, and 20 per cent., and these solutions have been used fresh to avoid any changes by the growth of fungi, &c. By means of a syringe the quantity of the solution used each time has been as nearly as possible 1 minim. The pupil has been measured by a Nettleship's pupillometer, or by means of a graduated thread, which could be easily placed across the cornea. During each experiment the subject was exposed as far as possible to the same light, so that the differences in size of the pupils should be as exact as possible.

The experiments on animals have been made strictly under the influence of chloroform, and in animals allowed to live after such experiments strict antiseptic precautions have been taken, so that apparently they scarcely suffered except from the after effects of the chloroform, the wounds being slight and quickly healed.

If only one measurement of the pupils of rabbits is given it is the transverse one.

Experiment I.

Starting first with the action of cocaine on the eye by conjunctival instillation, I found as the result of over one hundred observations on the human eye the following facts:—

The mydriasis is quickly attained and very large, and differs also from atropine in the pupil acting always to light and accommodation.

When the mydriasis is extreme, viz., 10 mm., the movement to light is very slight, and the initial contraction can only be seen by magnifying the pupil by the observer's putting on a pair of +5D spectacles. The slight contraction is immediately followed by a recoil which almost induces the belief that a dilatation has taken place.

The continuance of the mydriasis is comparatively short, the pupil attaining its normal size in from twelve to twenty-four hours. By drying the conjunctiva and cornea and placing the cocaine carefully on a limited part, the dilatation of the pupil will at first only take place at that spot, thus rendering the pupil irregularly dilated, and showing the limited action of the drug.

The following cases show the main points in the mydriasis of cocaine, and that the stronger the solution the quicker the initial and *ad maximum* dilatation.

I. H. C., 15. Pupils $6\frac{1}{2}$ mm.; cocaine 4 per cent. on right eye at 2.5 P.M.; at 2.25 P.M. pupil 10 mm., not further increased. The pupil had resumed its normal size twelve hours afterwards.

II. W. H., 30. Pupils 5 mm., cocaine 20 per cent. in left eye; in seventeen minutes pupil 8 mm., and other instillations did not increase its size.

Pupil regained its normal size ten hours afterwards.

III. E. W., 31. Pupils 5 mm., at 11.20 cocaine 2 per cent., at 11.30 cocaine 2 per cent., and at 11.50 pupils 8·5 mm.

Fourteen hours afterwards pupil normal size.

In rabbits the mydriasis induced by cocaine is, as a rule, very large, and in two cases I could not make out any action to light, but in both the pupil was 11·5 mm.; and the reason probably was the great stretching of the sphincter muscle of the pupil incapacitating its action; mydriasis was induced by conjunctival instillation, and also by injection into the anterior chamber.

Experiment II.—Cocaine and Atropine.

In cases of full cocaine mydriasis the application of atropine had no effect on the size of the pupil, but stopped its action to light and accommodation.

F. M., 25. Pupils 5·5 mm.; two instillations of cocaine 2 per cent. in right eye; pupil 9·5, acting to light and accommodation; atrop. sulph. $\frac{1}{5000}$ grain, no increase in size of pupil, although atropine put in three times, but the pupil did not act to light and accommodation.

Experiment III.—Atropine and Cocaine.

On adding cocaine to an eye fully under the influence of atropine,

an increase in the mydriasis occurs very quickly. The increase does not act to light or accommodation.

A. G., 16. Under atropine (4 grains to the ounce three times a day for ten days), both pupils 9 mm. and equal, not acting to light or movements of accommodation. In left eye cocaine 4 per cent., and in fifteen minutes pupil 10 mm.

In one case, however, in which atropine had been used for two months continually, the pupil was very much dilated, 10·5 mm., and here cocaine had no effect, the pupil being at its *ad maximum* dilatation.

F. G., 17. Has been using atropine for two months; pupils equal, regular, 10·5 mm., there being a very thin ring of iris visible; five applications of cocaine 4 per cent. in right eye, and no effect on the size of pupil.

The results of these two experiments led me to make a mixture of the two drugs, and by it an *ad maximum* dilatation of the pupil can be soon produced, which will not soon disappear like cocaine mydriasis.

The good effects of this in iritis were pointed out by me last January, and since have been fully sustained by increased experience.

E. G., 6. Pupils 5 mm., equal, act well to light and accommodation; at 2.24 P.M. a gelatine disk (of cocaine $\frac{1}{100}$ grain and atropine $\frac{1}{5000}$ grain) put on left cornea; at 2.29 P.M. no effect on pupil, slight drooping of lower lid.

2.34 P.M., pupil 9 mm. when shaded, 6 mm. in light, or on accommodating.

2.40 P.M., pupil 10 mm., acting slightly to light and accommodation.

2.42 P.M., pupil 11 mm., does not act to light and accommodation.

In this case, then, in eighteen minutes we had an *ad maximum* dilatation of the pupil. To test the difference in time between this and the action of the same quantity of atropine alone, the next experiment was done. This shows that in eighteen minutes no effect was noted on the pupil, and that in thirty-six minutes the pupil was only 9 mm., and the greatest dilatation attained was 9·5 mm.

E. G., 6. 2.46 P.M., right eye, pupil 5 mm., atropine disk $\frac{1}{5000}$ grain.

2.58 P.M., no effect.

3.4 P.M., no effect.

3.17 P.M., pupil 8 mm., does not act to light or accommodation.

3.22 P.M., pupil 9 mm.

3.45 P.M., pupil 9·5 mm., and did not increase.

Experiment IV.—Cocaine and Eserine.

The action of eserine on cocaine mydriasis was to induce myosis very easily. The myosis resembled that due to eserine alone.

On investigating more carefully I found that on taking 2 per cent.

solutions of each drug, and mixing them in the proportion of 25—28 of cocaine and ·1 of eserine, the normal size of the pupil was unchanged.

J. G., 22. Pupils regular, equal, 6 mm.; 4 instillations of solution of cocaine and eserine (27 : 1) in right eye. Pupil afterwards regular, 6 mm.

Experiment V.—Cocaine and Pilocarpine.

The action of pilocarpine was to induce myosis easily.

On trying mixtures of the drugs I found that 4 of cocaine and 1 of pilocarpine, each 2 per cent. solution, had no effect on the normal pupil.

A. W., 23. Pupils regular, equal, 5 mm., left eye pilocarpine, 2 per cent. on conjunctiva, bringing pupil to 2·5 mm.; 4 instillations of cocaine, 2 per cent., brought back pupil to 5 mm.

J. B., 30. Pupils regular, equal, 5 mm.; 3 instillations of cocaine and pilocarpine (4 : 1) had no effect on pupils.

The importance of these combinations I have utilised, especially that of pilocarpine and cocaine, for keeping the pupil a normal size during the operation of iridectomy, when anæsthesia has to be induced by cocaine. The objection to the dilated pupil is that it keeps dilated both during and after the operation, and so may become prolapsed afterwards, especially in glaucoma cases; also if a tolerably large conjunctival flap has been made the iris may disappear under it, and so become difficult to catch hold of, if iridectomy is to be performed. This combination does not prevent the local anæsthesia of cocaine. The antagonism between eserine and pilocarpine on the one hand, and cocaine on the other, gives us a means of estimating the relative power of pilocarpine and eserine on the pupil.

Experiment VI.

Cocaine increases the mydriasis produced by section of the 3rd nerve.

Experiment VII.

The effect of section of the 3rd nerve on a pupil dilated by cocaine stops its action to light and accommodation, but does not increase the size of the pupil.

Experiment VIII.

On stimulation of the 3rd nerve the mydriasis induced by cocaine easily and quickly gives place to myosis.

A. B., 29, suffering evidently from syphilitic mischief, probably gummatous, implicating the right 3rd nerve, and producing complete ptosis, external strabismus, inability to move the eye up, down, or in, diplopia, loss of accommodation and mydriasis.

Right pupil, 6 mm., does not act to light or accommodation.

Cocaine, 2 per cent., twice applied, and pupil 9·5 mm.

Black and White Rabbit.—Pupils 5·5 mm. × 6·5 mm., and act well. Skull cap removed, and 3rd nerves exposed. Right 3rd nerve cut; pupil = 6·5 mm. × 7·5 mm.; nerve then irritated by faradaic current, and pupil 3 mm. × 2 mm. Cocaine 2 per cent. twice put on right conjunctiva, and pupil dilated soon to 8·5 mm. × 7 mm., and then to 10 mm.

A weak stimulation of the 3rd nerve overcame the mydriasis, producing the same myosis as before whilst the stimulant lasted, and giving rise again to mydriasis.

Experiment IX.

On tapping the anterior chamber of an eye under the influence of cocaine, the dilated pupil contracts very little or not at all. This is, of course, contrary to what occurs in a normal eye, or in one under atropine, where marked contraction of the pupil occurs in all cases.

Experiment X.

On tapping the anterior chamber of a rabbit, and placing one electrode on the surface of the iris towards the centre, a weak faradaic current gave rise to contraction of the pupil in all cases of cocaine mydriasis, except two, in which very large mydriasis had been produced.

These latter, however, could be explained by the great stretching of the sphincter muscle, and hence stimulation directly of the fibres had no effect.

Experiment XI.

On the detruncated head of a frog the application of cocaine to the conjunctiva I have generally seen followed by mydriasis, a fact also seen independently by Dr. Waller, to whom I must express my thanks for numerous suggestions, and much help in my experiments. In my first case and two others I failed to get any effect on the pupil.

Experiment XII.

On the exsected eye of rabbits I have tried several times, with failure, to increase the size of the pupil by cocaine. In two cases, on injecting cocaine into the anterior chamber immediately after exsection, the contraction of the pupil due to tapping soon passed off, and the pupil became slightly larger than before tapping.

Experiment XIII.

On the eyes of rabbits which have been bled to death, cocaine gives rise to an increase in the mydriasis produced by the effect of the hæmorrhage.

Brown and White Rabbit.—Pupils 7·5 mm. × 6 mm.; right cervical sympathetic exposed and cut, very slight change in pupil; animal bled to death from ascending aorta in two minutes; pupils after death, right 7·5 mm., left 9·5 mm.; right sympathetic irritated by current, and pupil 10·5 mm.; 10.53, cocaine 4 per cent. twice to both eyes; 11.7, right eye pupil 9 mm., left eye pupil 10·5 mm.

In several experiments on the influence of cocaine on the mydriatic nerve which is to be found in the sympathetic accompanying the carotid in the neck, the following were the chief results:—

Experiment XIV.

On exposing the cervical sympathetic as it runs in proximity to the common carotid, cutting it and adding cocaine, dilatation of the pupil occurred in all cases, but less than the ordinary cocaine mydriasis, and in the first case scarcely at all.

I. Black Rabbit.—Pupils regular, equal, 6·5 mm. under chloroform; left sympathetic exposed, ligatured, cut, followed by slight contraction of pupil, and injection of vessels of ear, &c. The upper end of the nerve stimulated by weak faradaic current, and pupil 10 mm.

Right eye pupil 6·5 mm., left eye pupil 5·5 mm.; 10 A.M., cocaine 2 per cent. in both eyes; 10.8 A.M., right eye pupil 8·5 mm., left eye pupil 6 mm.

Cocaine 2 per cent. twice more in each eye; 11 A.M., right eye pupil 9·5 mm., left eye pupil 6·5 mm.

II. White Rabbit.—Pupils regular, equal, 6·5 mm. under chloroform; right sympathetic exposed, ligatured, cut; pupil afterwards 5·5 mm. On stimulation by weak faradaic current, pupil 10 mm.; 12.54 P.M., right eye pupil = 5·5 mm., left eye pupil 6·5 mm. Cocaine 2 per cent. twice on conjunctiva on each eye; 1.18, right eye 8 mm., left eye 9 mm.; 1.30, right eye 8 mm., left eye 10 mm.

Experiment XV.

On adding cocaine and producing mydriasis, subsequent section of the cervical sympathetic had no effect on the mydriasis, even when an *ad maximum* dilatation was produced.

Brown and White Rabbit.—Pupils 5 mm. × 6 mm. under chloroform; 10.35 A.M., left eye, cocaine 2 per cent.; 10.55 A.M., after three instillations of cocaine, left pupil 8 mm. × 7 mm.; 11.20, after five instillations of cocaine, left pupil 10 mm.

Left cervical sympathetic exposed, cut, and no effect followed on pupil, though all other symptoms of cutting the nerve.

Experiment XVI.

Cocaine mydriasis being produced, stimulation of the upper end of

the exposed sympathetic generally gave rise to an increase in the dilatation of the pupil.

On, however, carefully considering this, I found that it simply depended on the size of the mydriasis, as in cases where the *ad maximum* cocaine mydriasis was produced, stimulation of the sympathetic no longer increased it, though the strength of the current was increased. In some cases I found that the increase in the size of the pupil on stimulating the sympathetic was due to the fact that the animal was in a bright light, and therefore the pupil contracting to that light. On shading the eyes, and the pupil assuming its full dilatation, no extra effect was produced by stimulating the sympathetic.

I. Brown and White Rabbit.—Pupils 5 mm. × 6 mm. under chloroform, after five instillations of cocaine 2 per cent., left pupil 10 mm. On exposing the left cervical sympathetic, and stimulating it by faradaic current, no increase in mydriasis.

II. Brown Rabbit.—Pupils 7·5 mm. × 6 mm.; left eye, four instillations of cocaine 4 per cent., pupil 10 mm. On stimulating cut end by faradaic current, pupil 10·5 mm., being a very slight increase.

This, however, was done in a bright light, and on shading the eye, pupil 10·5 mm., and no increase no stimulating the cervical sympathetic.

Experiment XVII.

In three rabbits the cervical sympathetic was exposed, a piece taken out of it, and the wound stitched up, all the animals recovering without a bad symptom. The pupils on the operated side were contracted, but acted well to bright light.

In all, after a time, the result was the same, namely, no effect on adding cocaine, though applications often repeated, and of different strengths.

Large Brown Rabbit.—Pupils, under chloroform 9 mm. × 7 mm., act well.

Right eye, after 6 instillations of cocaine, 2 per cent., pupil 12 mm., and apparently they did not act to light.

On left side the cervical sympathetic exposed and 15 mm. of the nerve taken out, the upper end stimulated by faradaic current, and pupil became 12 mm.

The section was followed by contraction of the pupil to 7·5 mm. × 6·5 mm., and the other signs of cutting the cervical sympathetic. The wound was carefully stitched up and the animal soon recovered.

100 *Hours Later.*—Left eye, pupil oval, 6·5 mm. × 4·5 mm., but contracts well to light.

10.30, cocaine 2 per cent. on left eye.

This was repeated five times without effect, the pupil remaining the same.

Right eye, 5 instillations of cocaine, and pupil 12 mm.

Ten Days After.—Same experiment and same result.

Two Months After.—Left eye, pupil 6·5 mm. × 4·5 mm.; cocaine 4 per cent. three times, and no effect.

Right eye, same experiment, and pupil became 11 mm. The injection of the vessels of the ears, &c., still continues.

Brown Rabbit.—Pupils 7·5 mm. × 6 mm. under chloroform. Left cervical sympathetic exposed, and 10 mm. cut out, followed by dilatation of vessels of ear and slight contraction of pupil. Wound then stitched up four days after.

Left eye. Pupil: In bright light, 5.5 × 4 mm. In shade, 7 × 5·5 mm.

Right eye. Pupil: In bright light, 5·5 × 4 mm.; medium light, 7·5 × 6·5 mm.; shade, 8·5 × 7·5 mm.

2.30 P.M. Cocaine, 4 per cent., in both eyes.

2.35 P.M. Pupils the same. Cocaine, 4 per cent., repeated in each eye.

2.40 P.M. Left pupil in bright light, 5·5 × 4 mm.; in shade, 7 × 6 mm. Right pupil in bright light, 7 × 6 mm.; in shade, 10 × 10 mm.

Three other applications made, and same result.

Just as I had finished this paper I was enabled, through the kindness of Mr. Nettleship, to see a case of lesion of the cervical sympathetic on the right side, and am greatly indebted to him for allowing me to use the following notes. It will be seen that numerous applications of cocaine had not the slightest effect on the pupil.

B. P., 34. Married; for four years in bad health; seven months slight difficulty in swallowing, and slight dyspnœa; three months noticed difference in feelings of warmth on the two sides of the face —especially if excited; five months noticed pupils unequal in size, some fulness in middle of neck, but nothing definite to be felt. Little more pulsation in right carotid than in left.

V. Right : $\frac{6}{6}$: Jaeger i at 22 cm.
 Left : $\frac{6}{6}$: Jaeger i at 22 cm.

Right eye. Palpebral fissure smaller than the left. Media clear and normal.

Pupil.—Regular, no iritic adhesions, acts to light and accommodation, smaller than left; when right pupil is 2 mm., left is 3·5 mm. Looking at distant objects right pupil becomes $2\frac{1}{3}$ mm., when accommodating for 15 cm. it is 2 mm., and when shaded and accommodation relaxed, 3 mm.

Left eye. Pupil normal, acts to light and accommodation; when well shaded and accommodation relaxed it is 6 mm. On taking these notes, the one-sided flushing of the face was noticed. Six instillations of cocaine, 2 per cent., in the right eye, gave rise to no alteration in the size of the pupil.

To make certain of this result, Mr. Nettleship and myself made ten days afterwards the following observations on the patient:—

In a good light. 11.15. A.M. right pupil 2 mm., left 2·5 mm.; 1 disk of cocaine $\frac{1}{200}$ grain placed in each eye.

11.18, three more disks of same strength put in each eye.

11.25, right pupil 2 mm., left 4·5 mm.

11.35, right pupil 2 mm., left 6·5 mm.

11.55, right pupil 2 mm., left 7 mm.

12.10, pupils the same; on shading them the right pupil 3 mm., left 9 mm.

On now putting homatropine into the right eye the pupil dilated regularly as in a normal eye as below:—

12.15 P.M., homatropine (4 grains to the ounce) put in right eye; pupil was 2 mm.

12.30, right pupil now 6·5 mm., and does not act to light or accommodation.

All the preceding experiments pointed to the action of cocaine being an irritant of the sympathetic or mydriatic nerve of the eye, and this fact is proved beyond a doubt by the last experiment (XVIII). Both in rabbits in which a piece of the cervical sympathetic had been excised for some days, and also in a case presenting all the symptoms of section of the cervical sympathetic, cocaine had not the slightest effect on the pupil.

That such action is local and not central is proved by the extremely local and limited action of cocaine on all the parts of the body, including the pupil (Experiment I), and by dilatation of the pupil following its application to an exsected eye (Experiment XII). That cocaine mydriasis is not in any way dependent on the 3rd nerve is shown by Experiments VI, VII, and VIII, and also by the pupil acting to light and accommodation (Experiment I). That cocaine mydriasis is not due to paralysis of the sphincter muscle is shown by the ease with which eserine, which acts directly on the muscular fibre, reduces it, producing the usual myosis (Experiment IV), and also by the fact that stimulation of the muscular fibre directly gives rise to contraction of the pupil (Experiment X).

The cases in which this contraction on direct stimulation did not take place were due probably to the overstretching of the sphincter muscle. Thus by excluding the other means of producing mydriasis we have next to consider whether we can produce the same kind of mydriasis as that of cocaine by excitation of the sympathetic.

We find that the faradaic stimulation of the cervical sympathetic which will give rise to a dilatation of the pupil, at the same time acting to the reflex of light, is very weak, as any slight increase of the current stops this reflex action of the pupil. But that the local excitation of the sympathetic by cocaine and its induced mydriasis is

also weak is shown by the ease with which myotics (Experiment IV and V) and stimulation of the 3rd nerve (Experiment VIII) overcome the mydriasis.

Therefore our facts bring us to the conclusion that cocaine mydriasis is induced by a local irritation of the endings of the cervical sympathetic or mydriatic nerve in the eye. But this nerve consists of two distinct sets of fibres—one set acting on the blood-vessels as a vaso-constrictor, and the other only producing dilatation of the pupil. That such is the case can be proved by bleeding an animal to death when, even after several excitations of the cervical sympathetic, fresh stimulation of this nerve again increases the dilatation of the pupil induced by the bloodless state of the eye. François Franck, in his able monograph "On the Dilatator Nerves of the Pupil,"* brings forward several other conclusive arguments to prove the dual action of the cervical sympathetic fibres. That cocaine acts on the small blood-vessels by constricting them can be proved easily by its action on the vessels of the conjunctiva.

We may therefore assume that it also acts thus on the vessels of the iris—a fact which accounts for the diminution of tension in an eye under cocaine. But that this is not the only cause of cocaine mydriasis is evident from the following facts, viz., that the full mydriasis of cocaine is much larger than that induced by the bloodless state of the eye, and that cocaine increases the mydriasis induced by bleeding the animal to death (Experiments XI and XIII). It is the action on the purely mydriatic fibres that gives rise for a time to dilatation of the pupil by cocaine after section of the cervical sympathetic (Experiment XV). That this action of cocaine on the purely mydriatic fibres passes off after a time is shown on rabbits by Experiment XVII, and this is due to cutting off its communication with the mydriatic centre of Budge and Waller in the spinal cord.

The reason of this happening so soon is the weak stimulus due to cocaine, and therefore a very slight change in the nerve endings being able to prevent its action. That cocaine also acts on this mydriatic nerve is suggested by the enlargement of the palpebral aperture following its application. Hence from these experiments it appears that cocaine produces mydriasis by acting locally on the endings of the cervical sympathetic nerve in the eye, and also that it affects both sets of fibres, namely, the purely mydriatic and the vaso-constrictor.

* "Travaux du Laboratoire de Marey," Années 1877–79.

IX. "On the Occurrence of Glycogen as a Constituent of the Vesicular Cells of the Connective Tissue of Molluscs." By EDWIN RICHARDSON BLUNDSTONE, Scholar of Christ's College, Cambridge. Communicated by E. RAY LANKESTER, M.A., LL.D., F.R.S. Received June 2, 1885.

The following results were obtained in connexion with a research "On the Connective Tissue and Vascular System of Mollusca," on which I acted as assistant to Professor Lankester, according to the terms of a grant from the Government Grant Committee of the Royal Society: other results will be published subsequently.

The connective tissues of Molluscs, as presented by Helix, Planorbis, Anodon, Cyclas, and Solen, may be divided into two main groups. In one of these the constituent cells are little advanced from their original mesoblastic condition; they have an irregular stellate form, and they are joined together by the tips of their processes. In the other variety, which will be spoken of as lamellar connective tissue, the cells are more irregular in form and their processes more attenuated, but, by the deposition of an intercellular ectoplasm in certain planes, the cells come to lie in plates or films. These films form the walls of the sinuses and lacunæ (occasionally vein-like in appearance) of Molluscs. All the cells, however, of the lamellar connective tissue do not lie embedded in the films, for some of enormous size project into the blood, being only attached by a small portion of their superficies to the film.

These cells are the "vesicular cells" (Lankester) of the lamellar connective tissue: they contain *glycogen*.

1. *Tissues operated upon.*

In extracting glycogen from Anodon I have made use of the mantle, thus avoiding all contamination and complications of results on the part of the liver, and, by rejecting the mantle edge, ventral to the pallial muscles, have been able to work upon a region of the simplest composition, for with the exception of a very few muscular fibres which pass from side to side of the mantle, the region operated upon comprises only the two epidermes and the lamellar films with their glycogenous cells. The blood offers no difficulties, as it readily drains out of the mantle.

2. *Processes of Extractions, &c.*

In extraction I have made use of the process and tests detailed for investigating the liver of the rabbit for glycogen in Foster and Langley's "Elementary Physiology," and also of those in the admirable essay of Dr. Errera, to which I am much indebted.

To observe most of the following results, however, the more complicated processes alluded to above are not really essential. I therefore give a simpler process, whose results are *sufficiently* accurate.

The mantles of twenty Anodons (previously preserved in strong spirit) are treated, after removing the adherent spirit with blotting paper, with 3 oz. of boiling distilled water. While in the water, the mantles are disintegrated as far as possible with a glass rod. The mixture is freely shaken, and in a few minutes filtered. The filtrate is rapidly cooled, and then twice its bulk of absolute alcohol is added to it, and allowed to stand. The precipitate so obtained is taken up, after washing with 90 per cent. spirit, in distilled water, and again precipitated by absolute alcohol as before.

Upon dissolving this precipitate in distilled water a bluish opalescent solution is obtained which—

1. Gives mahogany colour with iodine solution—the colour disappearing upon warming, and reappearing upon cooling.
2. Gives no reaction with Fehling's solution upon warming.
3. After digestion with saliva at 30—35° for about 10 minutes, the solution gives Fehling's reaction.
4. Is precipitated by 60 per cent. alcohol.

The above is briefly the evidence of the occurrence of glycogen in the mantle of Anodon.

3. *The Localisation of the Glycogen in certain Cells.*

By a little care the mantle may be split in half (this is performed more easily with a spirit specimen). Placing one of the halves so obtained upon a glass slip, epidermis downwards, and treating the preparation with solution of iodine, a remarkable appearance is observed.

The tissues generally are hardly stained at all, but with the naked eye it is seen that the connective tissue is copiously sprinkled with dark brown dots. By the microscope these dots are found to be very large vesicular cells, some of whose contents have been deeply stained by the iodine.

4. *Some Reactions and Particulars of these Glycogenous Vesicles.*

For the study of these cells, thick sections of the frozen mantle of Anodon or preparations of the "mesentery" of Helix are best.

The vesicles are then seen to be very large round or oval cells, with very brilliant (though not doubly refracting) contents.

By treatment with water, the cells are emptied of contents except the nucleus and the cell protoplasm, which is very small in amount. By crushing, it is seen that the metaplasm ("endoplastic

product" Lankester) is fluid, and dissolves to an opalescent fluid in the surrounding water.

With strong spirit, the metaplasm undergoes a very remarkable and quite characteristic clotting or pseudo-crystallisation (also noticed by Errera), which takes place equally well either within or (in crushed specimens) outside the cell. Osmic acid yields no reaction.

It is the metaplasm or endoplastic product of the vesicle which is *deeply* stained by iodine, and also by borax carmine, and not the nucleus and cell protoplasm. I do not propose here to further describe the glycogenous vesicles, for they are described and figured, both fresh and after the action of various reagents, at great length by Professor Lankester and myself in the forthcoming paper to which allusion has before been made, but I will take this opportunity of pointing out—

1. That these "vesicles" are the same as the "plasma cells" of Brock and others, the "Langer's bladders" of very many writers, and are equivalent to many of the "lacunæ" of Kollmann, Griesbach, &c. For many years the existence of these vesicles has been denied and affirmed by two schools of observers; we shall bring forward indubitable proof in favour of Fleming and his adherents.

2. That these cells are trustworthily figured and described by Fleming ("Archiv für Mikros. Anat., vol. xiii), and may be readily seen in Anodon by the method described above, or in Helix by merely spreading out a portion of the "mesentery" on a glass slip.

5. *Distribution of the Vesicles.*

These vesicles occur in Anodon wherever there is lamellar connective tissue, except in the very muscular *tip* and *edge* of the foot, labial palpi and gills, Keber's organ, organ of Bojanus, mantle edge.

In Helix, they are found especially on the lining of the great lacunar spaces, and on the "mesenteries."

They are especially associated with the arteries in all Molluscs I have examined. The brilliant whiteness of the slug's arterial system is due to their presence in the connective tissue, *outside* the arteries.

I have reasons for believing that these cells, or slight modifications of them, are very widely distributed throughout the Invertebrata.

6. *General Remarks and Conclusion.*

Although glycogen has frequently been stated to occur in Invertebrates (*e.g.*, by Professor Foster for Ascaris, and by Fredericq for Mya), yet I believe that hitherto it *has never been definitely localised in certain cells*, and far from being associated with connective tissue, has been thought to be for the most part a liver product. I hope to be able to publish further results in the above directions at a future time.

In connexion with the Mollusca certain points seem worthy of special notice.

In the first place, it is held by many comparative anatomists that the lacunar system of Molluscs has a partly enterocoelous origin, or at least has enterocoelous elements in its nature. If this be so, it is interesting to note that some cells of the lacunar walls may be glycogenous, for glandular surfaces seem to be specially characteristic of the ectoderm and endoderm. Moreover, these cells are also to be found on the mesenteries of Holothurians, which are undoubtedly enterocoelous.

In the second place, one of the greatest objections which can be urged against the feasibility of water inception by Molluscs is removed, if (1) the specific gravity and (2) the nutritive quality of the blood can be maintained in spite of the process. It is supposed that this would be accomplished by the discharge of the contents of the glycogenous vesicles.

Finally it is interesting to note, that one of the functions of the vertebrate liver seems in Molluscs with ease to be performed outside its domain, and this, moreover, in animals whose liver is essentially a digestive gland.

In conclusion I have to thank Professor Lankester and Professor Foster, to whom, as also to Mr. Langley, Mr. Lea, and Mr. Gardiner, I am very greatly indebted.

X. "On the Development and Morphology of *Phylloglossum Drummondii*. Part I. Vegetative Organs." By F. O. Bower, M.A., F.L.S., Regius Professor of Botany in the University of Glasgow. Communicated by W. T. Thiselton Dyer, M.A., C.M.G., F.R.S. Received June 18, 1885.

(Abstract.)

The morphological history of *Phylloglossum* has up to the present time rested on a very slender basis. The following brief summary given by Sachs ("Textbook," 2nd English Edition, p. 463) practically comprises the whole of it:—"A small Australian plant, only a few centimetres high. It consists of a stem arising from a small tuber, and bearing at its lower part a rosette of a few long leaves, and one or more lateral roots; it is prolonged above this as a thin scape, and terminates in a spike of small leaves bearing the sporangia. The plant is propagated by means of adventitious shoots, consisting of a tuber with a rudimentary leafless bud; in this respect it resembles our native *Ophrydeæ*."

The study of so reduced a form of a group, usually so remarkable

for luxuriant vegetative development, seemed to promise a good deal of interest. Baron Ferdinand von Mueller, K.C.M.G., F.R.S., Government Botanist, Melbourne, with the unfailing energy which prompts him to assist every kind of botanical work, after one or two attempts succeeded in transmitting to the Royal Gardens, Kew, a parcel of tubers in a living condition. These were successfully grown for the first time in Europe. Their examination in the Jodrell Laboratory has yielded the results now communicated to the Society.

The mode of development depends to a certain extent upon the size of the tuber: where the tuber is small only vegetative organs are formed, where it is relatively large, the plant may form sporangia. Taking first the simpler case, it is found that outgrowths appear on the broad apex of the tuber, which is before germination a simple, smooth and rounded cone; these outgrowths are leaves; their number may vary from one to six or seven. They are arranged in an irregular whorl, of which the members on one side take precedence of the rest in time of appearance; they constitute in fact a "successive whorl." From the first they are rounded at the apex, and have no single apical cell. The apex of the axis, which has a central position at first, becomes gradually depressed, and is overarched by the surrounding tissue; it develops directly into the apex of the new tuber, which is accordingly of exogenous origin, and represents in this simpler case the actual apex of the parent plant. By a peculiar localisation of growth this apex becomes inverted, and by a process of development very similar to that of the axillary shoot in certain orchids (*e.g.*, *Herminium monorchis*), it projects laterally from the parent plant. Meanwhile an outgrowth appears on the opposite side of the axis from that on which the tuber projects, and below the insertion of the oldest leaf: this is the first root. It has been clearly proved, by both external observation and by study of sections, that the root in *Phylloglossum* is of *exogenous origin*. Among other known examples of this anomalous mode of root-development it is interesting to note the root of the embryo of *Isoetes*. In those cases where the tuber is relatively large, sporangia are formed: these are, as is already known, borne upon an elongated axis, which is the direct product of the apex of the tuber. A different origin is necessary in this case for the tuber, and it has been found that the tuber originates in such plants in an adventitious manner, as a depression at the base of the sporangium-bearing axis or peduncle: the details of its development are otherwise similar in this case to that above described.

A comparison of both external form, and as far as possible of internal structure, between *Phylloglossum* and the young plants of *Lycopodium cernuum* described recently by Treub, shows many points of striking similarity: this is so marked that the author draws the

following conclusion: that, provided the oophore generation of *Phylloglossum* (which has never yet been observed) corresponds in its more important points to that of *Lycopodium*, we may regard *Phylloglossum* as a form which retains and repeats in its sporophore generation, the more prominent characteristics of the embryo as seen in *Lycopodium cernuum*: it is a permanently embryonic form of a lycopodiaceous plant.

XI. "Researches on the Theory of Vortex Rings. II." By W. M. HICKS, M.A., F.R.S., Principal and Professor in Mathematics in Firth College, Sheffield. Received June 13, 1885.

(Abstract.)

The communication forms a continuation of some researches the first part of which was published in Part I of the Transactions for 1884. In that paper was considered the case of a circular hollow with cyclic motion through it. In the present the more general case is investigated where the core is of different density from the surrounding fluid, has a hollow inside it, and circulations additional to that due to the filaments of rotational fluid actually present. It does not seem to have been generally noticed that even in the case of the ring ordinarily considered, where the density of the core is the same as that of the surrounding fluid, and there are no additional circulations, the full theory ought to take account of the existence of a hollow, for when the energy of the motion (as was pointed out by the author*) is increased beyond a certain point, depending on the circulation and the pressure of the fluid where it is at rest, a hollow will necessarily begin to form. As it seems impossible to account for the very great differences in the masses of the various elements on the vortex theory of matter unless the cores are of different densities, the investigation includes the case where the density is arbitrary. As soon as the existence of a core is postulated the ring at once becomes more complex, depending on the density (or even the distribution of density) of its core, on its vorticity, and on the presence or absence of additional circulations. The vorticity has been taken uniform; this not only greatly simplifies the mathematical methods, but is also the case we should naturally choose first to investigate. In the general investigation the density is taken to be different from that of the surrounding fluid, the ring is supposed hollow, with an additional circulation round it, and another round the outer boundary of the core. It is evident that the presence of the former necessitates the

* "On the Problem of Two Pulsating Spheres," "Camb. Phil. Proc.," iii.

perpetual existence of the hollow. It is shown that the presence of the latter is necessary to render the ring stable when its density is greater than that of the surrounding fluid.

The investigation is divided into three sections. The first is preliminary, and deals with the necessary functions and their approximate values. The second is devoted to the state of steady motion ; and the third to the small symmetrical vibrations and the question of stability.

When the motion is steady the sectional centre* of the hollow lies outside that of the core. In general if C_1 is its position when the inner additional circulation is very large, and C_2 when the same quantity is zero, C_1 is outside of C_2, and the position of C when the additional circulation is anything else is the centre of gravity of masses proportional to the added circulation at C_1, and the circulation due to the core itself at C_2. When the hollow is just zero the distance of C_2 from the sectional centre of the core bears to the sectional radius the ratio $5r/8a$ where r is the sectional radius and a the aperture radius. This therefore is the point where the hollow begins to form when the energy is sufficiently increased. If with the same outer boundary the mass of the core be lessened (or size of hollow increased), both C_1 and C_2 move in, and ultimately coincide with the centre of the outer section.

So long as the core is only doubly continuous the volume is constant, and therefore the sectional radius varies inversely as the square root of the aperture radius. When there is no core it was shown in the former paper that the sectional radius of the hollow remained constant. In the general case, after a hollow is formed, the sectional radius of the core changes more slowly, and the additional circulations add to this tendency. The outer section always decreases as the aperture increases, but when the hollow becomes large this decrease is small, and the sectional radius of the core remains almost constant. The sectional radius of the hollow, however, increases with the aperture when the hollow is small, but decreases beyond a certain critical value. The expansibility of the ring, due to the presence of a hollow, has a marked effect on the variation of the velocity of translation with increasing aperture, the tendency being to make the variation smaller. With an internal additional circulation the ring will possess internal energy comparable with that of the external fluid. It will, however, decrease as the whole energy is increased. The fluted vibrations in general consist of two pairs, those of one pair travelling with the cyclic motion, and the other against, the mode being determined by the number of flutings. For a simply continuous core there are two times of vibration for each order; for a hollow core without internal

* By "sectional" centre, &c., is meant the centre of the cross section; by "aperture" centre is meant the centre of the aperture, &c.

additional circulation, there are three, together with a standing wave (corresponding to an infinite period). When both additional circulations are present the periods are given by a biquadratic equation.

When there is no rotational core the motion is always stable. When there is a doubly continuous core whose density is greater than that of the surrounding fluid there must in general be an additional outer circulation to render the ring stable. After a hollow is formed the simple ring usually treated of is still stable. But if the core is denser than the surrounding fluid, it is always stable only if the outer additional circulation is larger than a certain critical value, depending on the densities and the circulations. If it is less than this critical value, the ring becomes unstable at some point as the aperture increases.

XII. "Notes upon the Experimental Breeding of *Tænia Echinococcus* in the Dog from the Echinococci of Man." By JOHN DAVIES THOMAS, M.D. (Lond.), F.R.C.S. (Eng.), lately Physician to the Adelaide Hospital, and Member of the Council of the University of Adelaide. Communicated by T. S. COBBOLD, M.D., F.R.S. Received June 8, 1885.

The cystic parasite, *Echinococcus*, is known to occur under *four forms*, viz., as—

1. The Acephalocystic form.
2. Echinococcus scoliciparieus.
3. " altriciparieus.
4. " multilocularis.

As regards the *Acephalocystic form* first described in 1804 by Laennec, it is now generally regarded as a sterile variety in which either no brood-capsules and scolices have been formed, or in which such structures have once been present, but have degenerated and disappeared.

Echinococcus multilocularis is believed to owe its peculiar characters to the fact that it has developed, within tubular structures, *e.g.*, the *lymphatic canals* (Virchow), the *blood-vessels* (Leuckart), or the *biliary canals* (Schröder van der Kolk, Friedreich, and Morin).

It is not probable that this unusual growth is connected in any way with the biliary canals, because it has been found not only in the liver but also in the *lungs* and *peritoneum*.

Echinococcus scoliciparieus and *Echinococcus veterinorum* were long regarded as distinct species. Their distinctive characters being—

(*a.*) A marked difference in the mode of proliferation. In Echinococcus scoliciparieus brood-capsules and scolices being produced,

whilst in Ech. altriciparіens a more or less numerous progeny of daughter cysts is present. Within these, grand-daughter cysts, enclosing a third generation, may be found.

(*b*.) Küchenmeister alleged that the Echinococcus heads in these two forms, differ in the number, shape, and size of the hooklets. According to him Ech. scoliciparіens has twenty-eight to thirty-six hooklets of somewhat blunt shape, whilst Ech. altriciparіens possesses between forty-six and fifty-six of more slender figure and smaller size.

(*c*.) The habitat of the two forms is different, Ech. scoliciparіens occupying the viscera of the domestic Herbivora, whence it was also called *Ech. veterinorum*, Ech. altriciparіens, named also *Ech. hominis*, being the form found in man.

It is, however, very doubtful whether any of these differences can be regarded as specific, for great variations occur in the number of daughter-cysts present in Ech. altriciparіens, *e.g.*, there may be only one or two small cysts, or they may be reckoned by thousands. Again, as regards the characters of the hooklets, Rudolph Leuckart points out that considerable variety prevails in different individuals of the same species, dependent partly upon the age of the bearer, but also in part upon the circumstance that the shape, arrangement, and size of the hooklets are liable to considerable diversity.

Finally, Ech. scoliciparіens and Ech. altriciparіens may be found together in the same "host."

Obviously, the question of the identity or otherwise of these forms could only be satisfactorily proved by experiment. If the scolices of each form produced the same cestoid worm, no further doubt could remain as to the specific unity of the two varieties.

At this stage it is necessary briefly to allude to the evidence upon which our knowledge of the life-cycle of Ech. veterinorum depends.

Shortly after Küchenmeister had initiated his helminthological "Feeding Experiments" with Cysticercus pisiformis, Cœnurus, &c., experiments were made to discover the cestoid form of Echinococcus veterinorum.

In 1852 von Siebold,* by a series of successful feedings demonstrated that the tapeworm phase of Echinococcus was the small cestoid worm, now known as Tænia echinococcus.

The experiments of von Siebold were successfully repeated by Küchenmeister and others, and a very successful experiment was also made by Mr. Edward Nettleship, in 1866, an account of which was communicated to the Royal Society.†

* "Über die Verwandlung der Echinococcus-Brut in Tænien." "Zeitschrift für Wissenschaftliche Zoologie." Leipzig, 1853, p. 409.

† "Notes on the Rearing of *Tænia echinococcus* in the Dog from Hydatids, with some Observations on the Anatomy of the Adult Worm," by Edward Nettleship, Member Royal Agric. Coll., "Proceedings of the Royal Society," vol. 20, No. 86.

In all these cases, however, the dogs were fed with Echinococci obtained from the lower animals (Echinococcus veterinorum). As many able zoologists believed that there were two or more species of Echinococcus, it became a matter of considerable scientific interest, and indeed of great practical hygienic importance, to determine whether this really were so or not.

Both Küchenmeister and Zenker endeavoured to breed the adult tapeworm from Echinococci derived from man, but without success.

In 1863, however, Krabbe and Finsen in Iceland, as well as Naunyn at Berlin, succeeded in breeding Tænia echinococcus from the hydatids of man, but for reasons to be mentioned hereafter, their experiments can scarcely be regarded as decisive.

In order that experiments of this kind may be considered as satisfactory, certain conditions must be fulfilled. It is necessary—

1. To employ animals which could not be suspected of having been infected prior to the time of experiment, or to take steps to remove any entozoa that might be present, by the previous administration of suitable vermifuges.

2. To place the animals under circumstances in which it would be impossible for them to acquire infection from any accidental source.

3. To feed them with the *living* Echinococci of man.

4. To ensure complete success it is necessary not only that the tapeworm should be found in the intestine of the dog, but also that it should be found in numbers proportionate to the number of Echinococcus heads administered, and, moreover, the stage of development reached by the tapeworms should correspond with the date of the experimental "feeding."

5. No kinds of tapeworms should be present excepting such as can be satisfactorily accounted for.

Judged by this standard, neither the experiments of Krabbe nor those of Naunyn can be regarded as decisive.

Six separate experiments were performed by Krabbe* and Finsen, the number of dogs employed being *nine*.

Experiments Nos. 2, 3, and 5 were entirely unsuccessful, no specimen of Tænia echinococcus being found in the dogs. These experiments included four out of the total number of nine dogs observed.

Experiment No. 1.—In the autumn of 1862 M. Finsen sent to M. Krabbe a glass containing Tæniæ found by him in the intestines of a young dog, to which *some months previously* he had administered Echinococci obtained from a patient operated on by Recamier's method.

These Tæniæ were chiefly specimens of Tænia cucumerina, but there was found also *one mature joint of Tænia echinococcus.*

* "Recherches Helminthologiques en Danemark et en Islande," par H. Krabbe, Copenhagen, 1866, p. 49, *et seq.*

This case is not entirely satisfactory, since no mention is made of any precautions having been taken to prevent infection of the dog in question, from other sources than the experimental feeding, and *as dogs in Iceland are very frequently infested with Tænia echinococcus*,* the experiment cannot be regarded as decisive. Besides, the feeding took place *some months previously*, and as the sojourn of Tænia echinococcus in the intestine of the dog is believed not to exceed about two months, this renders the experiment still less conclusive.

Experiment No. 4 was by far the most successful one performed by MM. Krabbe and Finsen. On this occasion *two* dogs, which had been kept under close surveillance by M. Finsen since they had ceased suckling, were fed with the Echinococci of man upon two separate occasions at an interval of a month. *Three months* after the second feeding, they were examined. *One gave a negative result;* the other contained a moderately large number of Tænia echinococcus and four examples of Tænia canis lagopodis.† This experiment would be a very satisfactory one, if the presence of Tænia canis lagopodis could be accounted for; but failing this, some doubt must exist as to whether some accidental infection had taken place or not.

Experiment No. 6.—*Two dogs* were fed: *one yielded a negative result.* In the other there were found *four* specimens of Tænia echinococcus, in company with *four hundred* of Tænia canis lagopodis. The remarks made upon Experiment No. 4 apply with greater force to this observation. It will thus be seen that out of *nine dogs* experimented upon *only three* yielded any specimens of Tænia echinococcus. In one of these only one mature point was found; in another only four examples of Tænia echinococcus to one hundred times as many of Tænia canis lagopodis.

Naunyn's experiments were performed in Berlin in 1863.‡

He administered to *two dogs* the contents of a hydatid of the liver which contained Echinococcus heads, and which had been obtained from a woman operated upon by puncture of the cyst. *One of the dogs*, which was examined twenty-eight days subsequently, *contained no intestinal worms;* but in the second one Naunyn found, on the thirty-fifth day after the feeding, examples of Tænia echinococcus from one to one and a half lines in length. The chief drawbacks to

* Krabbe, during a visit to Iceland in 1873, examined 100 dogs procured from various parts of the country, and found *Tænia echinococcus* in 28 instances.

† Krabbe found this parasite in more than *one-fifth* of all the Icelandic dogs (over 100 of all ages) examined by him. It seems to be a peculiar species, whose genetic history is not yet known. Krabbe and Cobbold regard it as identical with the *Tænia litterata* of Batsch. Be that as it may, its frequent presence in Icelandic dogs seriously vitiates the results of the experimental feedings of M. Krabbe.

‡ "Ueber die zu *Echinococcus hominis* gehörige Tænie," Reichert und du Bois-Reymond's Archiv für Anatomie und wissensch. Medicin, 1863. Heft IV, p. 412.

Naunyn's experiments are, the complete failure in one instance, and the apparent absence of any precautions to prevent prior accidental infection in the other case.

The Author's Experiments.

On 25th June, 1883, I purchased four puppies, which were said to be a cross between a bloodhound and a mastiff. At the time they came into my possession they were seven weeks old, and up to that moment they had never left a small enclosed yard, in which their mother was chained. Beyond the maternal milk, they had received no food, except cooked scraps from the kitchen of their owner, and indeed very little of anything except their mother's milk.

On the evening of June 25th they were enclosed in a recently disused powder magazine, situated on the North Park Lands, Adelaide. The magazine in question is now the property of the University of Adelaide, and its use for the purpose of these experiments was kindly granted to me by the Council of the University.

During the seclusion of the dogs, they received only *cooked food* from my own kitchen, and *rain water* collected from the roof of the magazine, and stored in a new tank within the magazine enclosure.

The animals remained in good health and grew rapidly.

Experiment No. 1.—On July 5th I aspirated a hydatid tumour situated in the epigastrium of a woman, who was then a patient under my care in the Adelaide Hospital.

Two ounces of *nearly clear* hydatid fluid were removed, and as I thought I could see with the naked eye a few groups of scolices floating in the fluid, I administered it, mixed with milk, to one of the dogs. The feeding took place about two hours after the fluid was removed from the human body. The dog continued in good health, and was killed by prussic acid on August 5th, *i.e.*, on the 31st day after the feeding. *No tapeworms* were found, but several ascarides were present in the small intestine.

This experiment was therefore quite negative in its results; but as the patient had been some years previously operated on for the same hydatid, and as consequently *it is very doubtful whether any living scolices were administered*, it was used only in establishing the original freedom of the dogs from Echinococcus.

Experiment No. 2.—For some months it was impossible to procure any living scolices, but on October 16th I fortunately met with a suitable case. The patient, a healthy young man, aged twenty-five, was found to have a hydatid cyst on the upper surface of the liver, which was aspirated by me on October 16th, 1883. Twelve ounces of perfectly normal hydatid fluid were removed. Upon microscopic examination, a considerable number of scolices, mostly collected into groups, were found.

The fluid was allowed to stand in a decantation glass, protected from dust by a glass shade, for a couple of hours, and the bulk of the supernatant fluid was then poured off. Three and a half hours after its removal from the man, about half the residue, containing many groups of scolices, was poured down the throat of one of the dogs; most of it seemed to be swallowed, but some was coughed up. Milk was then poured down the dog's throat, and afterwards a further quantity of milk was supplied in a vessel; this was readily lapped by the animal.

On the next day, October 17th (about sixteen hours after the fluid was obtained), most of the remaining scolices were administered, being poured down the throat of the dog in about an ounce of their native fluid. As before, a supply of milk followed, this time with the addition of breadsops. Twenty-nine hours after the removal of the hydatid fluid, I examined some of the contained scolices, and found a considerable portion of them alive and moving actively on the hot stage (temp. 100° F.). On November 18th (*thirty-two days after the feeding*) the dog was killed with chloroform, and the small intestine and stomach were carefully examined by Dr. Whittell and myself. No appearance of Tænia echinococcus could be recognised on the lining of the intestine, but a great number of Tænia cumumerina were noticed, partly in the lower portions of the upper half of the small intestine, and in greater numbers in the lower half. No round worms could be found. The lining membrane was washed with warm water, and the washings were then carefully examined.

In the washings of the upper half we found *about* 100 *specimens* of *Tænia echinococcus*, out of which about eighty complete worms were collected; many terminal points also were seen. In the washings of the lower half of the bowel, I found six complete specimens. These Tænia were evidently unripe, and none of them showed four points, or indeed proglottides containing ripe hard-shelled eggs. Many of them had three segments, including the head, but not a few consisted only of a head, unsegmented neck, and one terminal proglottis.

In the terminal joint, the cirrus, cirrus-sac, and coiled vas deferens were seen, and in some cases faint indications of egg-formation. The head showed the usual suckers and rostellum, and in several cases a double circlet of hooklets.

The worms varied from about $\frac{1}{12}$ to $\frac{1}{8}$ inch (one to one and a half lines) in length, and were so fragile that a portion of the brood, carried carefully in a bottle containing camphor water for a distance of a few miles, were found by Dr. Whittell to be, without a single exception, broken into separate segments.

The circlets of hooks were usually absent or incomplete, probably in consequence of mechanical injury, but in one instance a complete crown was found.

However, in consequence of the position of the head of this specimen on the slide, it is difficult to accurately count the hooks. They appear to be from thirty-five to forty in number.

The dimensions of the hooklets, as measured by Dr. Whittell, were: —Maximum length, $\frac{1}{950}$ inch; length of root-processes, $\frac{1}{1420}$ inch; greatest transverse diameter at junction of claw with root-processes, $\frac{1}{2500}$ inch. The root-processes were well developed. It should be mentioned that on October 14, *i.e.*, four days before the date of the examination, this dog was allowed by a careless attendant to escape for twenty-four hours from the magazine; but as it was known to have concealed itself on the premises of a neighbour for the whole of the time, and as it was impossible for it to have procured any meat in this period, the experiment cannot be regarded as vitiated. Besides the short space of time (four days) that intervened between the temporary escape of the dog and its examination, renders it certain that the specimens of Tænia echinococcus found were the result of the experimental feeding thirty-two days before.

As regards the specimens of Tænia cucumerina present, these owed their origin to dog-lice swallowed by the animal, and the lice themselves were no doubt communicated to the dog in its babyhood by its mother.

Experiment No. 3.—A third dog was, on November 16, 1883, fed upon a considerable number of daughter cysts and scolices procured from the body of a woman who died of phthisis at the Adelaide Hospital on the afternoon of the previous day. A further "feeding" from the same source was administered on the morning of the 17th. Living scolices were found present in a portion of this "feeding" ten hours after the rest had been given to the dog. On December 10, 1883, Dr. Dunlop, of the Adelaide Hospital, supplied me with about 8 ounces of nearly clear hydatid fluid containing a great number of scolices. The fluid had been obtained from two hydatids occupying the pelvis of a woman who had died on December 9th. A considerable dose of the scolices mixed with milk was given to the same dog twenty-five and a half hours after the decease of the patient. This "feeding" contained some living scolices, but the majority of those examined upon the hot stage exhibited no movement when warmed, and hence were probably dead. On December 30th, *forty-four days* after the first feeding, and *twenty days* after the second dose of scolices, the small intestine of this dog was examined by Dr. Whittell and myself. Contrary to expectation, we found no ripe Tænia echinococcus; but after a patient examination we found *one complete*, but juvenile specimen of this worm, and the terminal joints of *seven* others. These were quite young, being very small (about one-twelfth inch when complete), and extremely fragile. However, the specimens showed plain indications of the generative organs, but no distinct

egg-formation. It was evident that the Tæniæ present were the result of the second feeding, so that they were twenty days old. The only complete worm found had three distinct joints (including the head), but it had lost its hooklets, probably from the endosmosis of water.

It is likely that in the case of both "feedings" most of the scolices were either dead or dying, for the fluid in which they were contained showed signs of commencing decomposition. This probably accounts for the small number of Tæniæ found in the dog. Besides the specimens of Tænia echinococcus, there were some hundreds of Tænia cucumerina, and one small ascaris in the small intestine.

Experiment No. 4.—In this case I thought it advisable to vary the mode of experiment, and to administer a vermifuge to the dog prior to the experimental feeding. As I could not recall a single instance in which I had found Tænia cucumerina absent from the intestines of the stray dogs examined by me in Adelaide, it appeared to me that the absence of this worm in a dog, after the administration of a vermifuge, would be good presumptive evidence that all other entozoa had been also removed by the medicine.

A male black and tan terrier which had been in my possession for three or four weeks, enclosed in the powder magazine, received on May 27, 1884, 30 grains of kamala in milk, and two days later a second similar dose was administered. On June 1st the dog seemed to be well, and a dose of echinococcus heads (procured by puncture of a hydatid cyst of the right lung in one of my patients) was administered in a little milk.

On July 13th, *forty-two days* after the feeding, the dog was poisoned with prussic acid and examined.

The small intestine contained semi-fluid brown matter apparently more "bilious" than usual, and the lower end of the small intestine was unusually red, and apparently congested.

After a careful search I found one specimen of Tænia echinococcus; it was quite perfect, and when first examined, it showed a contraction at the neck, apparently having a head, a short neck, and three succeeding segments. The contraction in the neck shortly disappeared, and when mounted in glycerine and stained with picro-carmine, the worm contracted greatly, especially in length. Faint indications of ovum-formation were visible in the posterior segment.

A drawing of this specimen was made by Dr. Dunlop immediately after its removal from the dog's intestine.

A very close examination of this dog's intestine was made, and there were found a few very small and evidently juvenile round worms, and the heads and necks of many Tæniæ cucumerina. These young tapeworms rarely exceeded a quarter of an inch in length, and none were found over one-third of an inch long. There was not a single worm with distinct proglottides.

It seemed clear to me that all the entozoa present were bred after the effect of the doses of kamala had passed away, and consequently that the specimen of Tæniæ echinococcus was forty-two days old, and the product of the experimental "feeding."

Summary.

In three out of the four dogs experimented upon, Tænia echinococcus was found, and in the unsuccessful case it is doubtful whether *living* scolices were given to the dog.

The dogs were examined at various periods after the feedings, viz., 20, 32, 42 days, and the Tæniæ found corresponded in stage of development with the interval of time that had elapsed since the feeding.

I am indebted to Dr. Dunlop, of the Adelaide Hospital, for the drawings of the Tæniæ bred in the experiments recorded, and also for the illustrations of ripe Tæniæ procured from stray dogs, examined by me in Adelaide.

XIII. "Note upon the frequent Occurrence of *Tænia Echinococcus* in the Domestic Dog in certain parts of Australia."

By JOHN DAVIES THOMAS, M.D. (Lond.), F.R.C.S. (Eng.), lately Physician to the Adelaide Hospital and Member of the Council of the University of Adelaide. Communicated by T. S. COBBOLD, M.D., F.R.S. Received June 4, 1885.

From the well-known prevalence of hyatid disease (both in man and the domestic herbivora) in certain parts of the Australian continent, it might naturally be expected that *Tænia Echinococcus* should be often found in the domestic dogs of these parts, but as far as I know, no investigation had been made by any observer with a view to the determination of the point in question.

Accordingly, towards the close of the year 1882, I commenced an inquiry into the matter, and although I have been prevented by the claims of my daily professional duties from an exhaustive investigation of the point, yet the facts already ascertained fully account for the very serious prevalence of hyatid disease in the localities where investigation has been made.

Dogs were examined at the following places in South Australia:—

1. In the city of Adelaide; 2. At Mount Gambier; 3. At Millicent; 4. At Penola.

The three last-named places are situated in the south-eastern district of the province, which is the most highly infected part of Australia as regards hyatid disease.

In all four places *Tænia Echinococcus* was found in the stray dogs captured by the police.

1. *Adelaide.*—In the first instance, *twenty* dogs were examined here. *Tænia Echinococcus* was found in *nine*, and in numbers varying from a few specimens up to many thousands. (Microscopic specimens of some of these accompany this paper.)

2. *Mount Gambier.*—*Five* dogs were examined here on December 7, 1882. *Two* contained numerous examples of *Tænia Echinococcus.*

3. *Millicent.*—Out of *four* dogs examined, *one* contained this parasite.

4. *Penola.*—The single dog examined at this place had thousands of specimens of *Tænia Echinococcus* in its small intestine.

Including all the dogs examined in 1882 (thirty in number) *no fewer than* 40 *per cent.* were infested by this dangerous tapeworm.

However it should be stated that nine dogs have been examined by me more recently, and that in only one of these could I find *Tænia Echinococcus.* This series of dogs differed from the former one in some respects. The first group of dogs consisted of vagrant and ownerless animals that must have picked up their living largely from slaughter-houses, butchers' shops, and offal heaps. These naturally would have many opportunities of eating the viscera of slaughtered animals, cast aside as unfit for sale. When the second series was procured, a very energetic raid against unregistered dogs had been made by the police for several months, consequently many of the dogs of this group had owners, by whom they were fed, but who were unwilling to pay the registration fee required by law.

In Melbourne.—*Ten* stray dogs collected in the town of Hotham (which is really a part of Melbourne) were examined on January 15th and 16th, 1883. *Five* of them contained *Tænia Echinococcus* in greater or less number. These observations show a very alarming prevalence of this dangerous parasite in the places where inquiry into the matter has been made, and fully explain the frequency of the cystic form (hyatid) both in man and the domestic herbivora of these localities.

In connexion with this subject, it may be mentioned that, according to Cobbold, this tapeworm has never been seen in any English dog which had not previously been experimentally infected.

Even in Iceland only 28 per cent. of the dogs examined by Krabbe were found to contain this tapeworm.

XIV. "Preliminary Note on the Nephridia of a New Species of Earthworm." By Frank E. Beddard, M.A., F.R.S. (Ed.,) Prosector to the Zoological Society. Communicated by Professor E. Ray Lankester, M.A., LL.D., F.R.S. Received May 19, 1885.

The specimens upon which the following observations are based I owe to the kindness of Prof. T. Jeffrey Parker, of Otago University; I received from him a large number of examples of an earthworm which appears to belong to Perrier's genus, *Acanthodrilus*,* though differing from any of the species of that genus as yet described. In the present paper I do not intend to describe the general anatomy of this worm, but to refer merely to the disposition of the "segmental organs" or nephridia, which present certain interesting peculiarities which have not, to my knowledge, been recorded in any other earthworm.

Each of the segments of the body in this species, instead of possessing only a single pair of nephridia, is furnished with four pairs, a single nephridium corresponding to each of the eight setæ; the setæ are not disposed in four series of pairs as in *Lumbricus*, but in eight longitudinal rows of a single seta, each separated by nearly equal intervals. On making a dissection of a large example (12 inches in length) it was quite easy to observe the two nephridia of the dorsally placed pair of setæ, and to trace by help of a lens the duct which perforates the body-wall in the immediate neighbourhood of the setæ; each of these nephridia appeared to be quite distinct from its neighbour; the nephridia belonging to each of the ventrally placed pair of setæ, on the other hand, form a continuous mass closely adherent to the intersegmental septum.

On making a series of transverse sections the appearances observed by a naked-eye inspection were confirmed; a nephridial tube perforates the body-wall and opens on to the exterior in the neighbourhood of each of the eight setæ. In every case the nephridium passes up close to the seta and is imbedded in the loose connective tissue which fills up the gap in the longitudinal muscular coat partly occupied by the seta and its special muscles. At the junction of the longitudinal and the outer, circular, muscular coat the tube bends at right angles and passes in a direction parallel to the fibres of the circular coat, but below it, *i.e.*, between the two muscular coats; this portion of the nephridial tube is considerably longer in the case of the extreme dorsal nephridium—about three times as long. The tube then passes in an oblique direction through the circular coat and opens on to the

* "Nouv. Arch. d. Muséum," t. viii.

xterior by a very minute orifice. Although the orifice is so small its presence is readily detected by the alteration in the character of the epidermic cells; the large, oval, glandular cells entirely disappear, and the narrow columnar cells become more closely packed together, and bend over towards each other on either side of the orifice, precisely as at the points where the setæ protrude on to the exterior of the body. The whole of the nephridium appears to be composed of a variously coiled tubule consisting of rows of cells placed end to end and perforated by the duct, as Claparède has described in *Lumbricus;* in the present species, however, unlike what is found in *Lumbricus*, the terminal portion of the nephridium does not differ at all from the rest, except at the extreme distal end, where it appears that the lumen ceases to be intracellular. This section of the tubule, which is very slightly wider than the rest, is surrounded by a flattened epithelium consisting of very small cells, and appears to be lined by a continuation of the chitinous cuticle which covers the exterior of the body; this last fact, however, I am unable to state with certainty, though it is probable for other reasons. I have observed the appearances described above in a very large number of sections taken from different regions of the body.

I propose at some future time to publish a fuller description, accompanied by figures, of these facts, which have some bearing upon the morphology of the Annelida.

It was supposed for a long time that only a single pair of nephridia were to be found in each segment of the segmented worms. Dr. Eisig,* however, found that in certain Polychætous Annelids belonging to the family Capitellidæ, there were frequently four, five, or even six pairs of nephridia in many of the segments of the body, and that the number of pairs increased from before backwards. In the present paper I have been able to extend Dr. Eisig's discovery to the Oligochæta, though the relation between the disposition of the nephridia in the two groups is not at all a close one.

In the Capitellidæ the nephridia of each segment are deposed in an oblique line passing from near the ventral parapodium of each side to near the nerve cord; their orifices, which, as in most Polychæta, are placed upon the summit of a tubercule, are grouped together in an irregular fashion near to the parapodium; moreover, the young Capitellidæ possess a *larval series* of nephridia—one to each segment—which are replaced by the nephridia of the adult. It is, therefore, a matter of great difficulty to institute a strict comparison between the nephridia of the *adult* Capitellidæ and those of *Acanthodrilus;* I can only call attention to the general fact that *the Oligochæta, like the Polychæta, may possess more than a single pair of nephridia in each segment.*

* "Mitth. Zool. Stat. Neapel," Bd. i.

A discussion of certain other questions in the morphology of earthworms, naturally arises out of the foregoing facts. Professor Lankester, in a memoir upon the anatomy of *Lumbricus*,* called attention to the fact that there is a constant and definite relation between the nephridial aperture on the one hand, and the apertures of the genital ducts on the other hand, to the setæ; the nephridia open close to the ventral pair of setæ, the genital ducts and copulatory pouches have a similar relation to the dorsal pair. Claparède had previously expressed the opinion that in the Limicolous Oligochæta, the ducts of the genital system were the modified representatives of the nephridia, basing this opinion on the general similarity of structure and position, and more especially upon the fact that nephridia are absent in those segments of the body which contain the genital ducts; he denied, however, that this comparison could be applied in the case of earthworms, since here the genital ducts coexist in the same segments with nephridia. Professor Lankester, in the paper just referred to, pointed out that Claparède's views could be extended to earthworms, if it were admitted that each segment of the body were typically furnished with two pairs of nephridia, one corresponding to each of the pairs of setæ. Professor Lankester supposed that in *Lumbricus* the copulatory pouches and genital ducts were the modified representatives of the second series of nephridia, which had disappeared elsewhere than in the genital segments. Certain facts in the anatomy of other genera of earthworms, made known by the researches of M. Perrier, appeared to lend very strong support to this hypothesis. M. Perrier† has described certain earthworms (*Acanthodrilus, &c.*) which agree with *Lumbricus* in that the nephridia open on to the exterior of the body by the ventral setæ; in others again (*e.g. Anteus, Rhinodrilus*) the nephridia are related to the dorsal pair of setæ; finally, in *Plutellus*,‡ there is an actual alternation in the position of the nephridial orifices, in some segments they open near to one of the dorsal pair of setæ, in others near to one of the ventral pair. All these facts seem to be best explained by supposing the typical presence of two series of nephridia, a dorsal and a ventral, one or other of which has partially or entirely disappeared; in *Lumbricus* the dorsal series has disappeared, in *Anteus* the ventral series; while in *Plutellus*, both dorsal and ventral series have partly persisted. Although this hypothesis serves to explain the series of facts which have been briefly stated above, M. Perrier has come to the conclusion that it cannot be adopted in the way that Lankester has suggested, inasmuch as there are reasons for disbelieving in any homology between the nephridia and genital ducts. If the orifices of

* "Quart. Journ. Micr. Sci.," 1865.
† "Nouv. Arch. d. Muséum," t. viii.
‡ "Arch. d. Zool. Exp.," t. ii.

the nephridia were always constant to a series of setæ different from that to which the orifices of the genital ducts belong—as in the case of *Lumbricus*—then it would be permissible to retain the hypothesis of two series of nephridia. Such, however, is not the case; in *Urochæta* and in *Plutellus*, the orifices of the copulatory pouches coincide at the same seta as the orifices of the nephridia; and it also difficult to explain the fact that the vasa deferentia traverse several segments, all furnished with distinct nephridia, on their way to the exterior.

It appears to me that the coincidence of nephridium and copulatory pouch at the same series of setæ is not a serious objection to Professor Lankester's hypothesis. It is extremely possible that the copulatory pouch may be the equivalent of a diverticulum of the duct of the nephridium; such diverticula I have myself described in a large earthworm (*Microchæta*) from the Cape Colony in a paper read before the Zoological Society, and they exist elsewhere. In *Microchæta* there are no proper copulatory pouches; these structures appear to be functionally replaced in four segments of the body by several extremely minute cæcal pouches placed in the immediate neighbourhood of the nephridium of their segments. M. Perrier* has described in a species of *Perichæta* similar accessory pouches having a precisely similar relation to the copulatory pouches; and in my paper I have called attention to the great resemblance between the copulatory pouch of this *Perichæta* with its independent accessory pouches, and the nephridium of *Microchæta* with its large muscular diverticulum and the series of similar cæcal pouches. I have also illustrated the comparison by figures, which will be shortly published.

Whatever may be the value of M. Perrier's criticisms of Professor Lankester's hypothesis, it is quite clear now that there is no intrinsic improbability in this hypothesis. The new facts that have been brought forward in the present paper also serve to render intelligible certain other facts in the morphology of earthworms.

In his earliest paper on the anatomy of earthworms, M. Perrier adopted the hypothesis of two series of nephridia, one corresponding to each *pair* of setæ in those earthworms that are furnished with four pairs. In *Lumbricus* the two setæ of each pair are closely approximated, but in many other earthworms, as in the *Acanthodrilus*, which forms the subject of the present communication, the two setæ of each pair become widely separated, so that there are eight longitudinal series of a single seta each; in these cases it is important to know what becomes of the nephridial orifice; has it, in fact, any definite relation to one or other of the two setæ of the pair? M. Perrier has addressed himself to this question, and has recorded the fact that in *Anteus*, *Rhinodrilus*, and *Moniligaster*, where the ventral series of nephridia are alone present, their orifices are situated close to the

* "Nouv. Arch. d. Muséum," t. viii, Pl. IV, fig. 72.

outermost of the two setæ which compose the pair; in the genus *Titanus*, where the nephridial orifices are dorsal in position, they open close to the outermost of the two dorsal nephridia. There thus appears to be a constant relation between the nephridial apertures of both the dorsal and ventral series to the outermost seta of each pair. His investigations into the anatomy of *Plutellus* led M. Perrier to abandon this position. In *Plutellus* the setæ are arranged, as in the genera just referred to, in eight longitudinal rows, and it has been already stated that the nephridia themselves alternate from segment to segment, sometimes opening by one of the dorsal, at other times by one of the ventral setæ; there is, however, no constancy to any one in particular of the two setæ which compose the pair; occasionally the nephridial orifice will be found close to the outermost, and occasionally close to the innermost, of the two setæ of its pair. These facts are clearly not explicable by assuming the typical presence of two series of nephridia in earthworms, but they become perfectly clear and intelligible in the light of the facts that I have been able to bring forward in the present paper; in *Plutellus* there are the remains not only of two series of nephridia, but of four, each corresponding to one of the four rows of setæ of each half of the body.

We are therefore now in a position to extend Professor Lankester's hypothesis, and to assume *that to each seta, and not to each pair of setæ, corresponds a separate nephridium.* No doubt Professor Lankester's hypothesis is correct so far as it goes; it is very probable that when the two setæ of each pair come to be placed close together, one of the two nephridia disappears.

Another question raised by the foregoing facts concerns the general problems relating to the distribution of the setæ in earthworms; are we to regard the presence of four series of pairs of setæ as typical for earthworms, or is it possible that this condition has been arrived at by a process of reduction, the primitive condition being a complete ring of setæ round each segment, as is actually found in *Perichæta?*

There are a good many facts which appear to support the former alternative; (1) the undoubtedly close resemblance between the two pairs of setæ of an earthworm, and the dorsal and ventral parapodia of a Polychætous worm; (2) the fact that in the young *Perichæta* the setæ are by no means so numerous as in the adult; we owe this observation to M. Perrier, but unfortunately he has not recorded the exact number of setæ, and whether there was any real approximation to the quadriserial disposition.

Other facts point to the latter alternative. In *Urochæta* the setæ are disposed in eight longitudinal rows, but in the posterior part of the body the setæ do not exactly correspond in position in a series of segments; there is in fact a quincuncial arrangement; in the interval

between the setæ of a given segment are certain peculiar bodies having the appearance of unicellular glands, and surrounded by an inward prolongation of the cuticle. Quite similar structures have been met with by Vejdovsky in *Anachæta*,* and in this worm *they replace the setæ;* there is therefore evidently some reason for supposing that *Urochæta* has descended from ancestors in which there was a complete row of setæ round each segment.

M. Perrier is of opinion that the quadriserial arrangement referable to the parapodia of the Polychætous worm is the primitive arrangement, and that the condition found in *Perichæta* may have been arrived at, first, by a separation of the setæ of each pair, and secondly, by the development of the secondary setæ ("*soies de remplacement*"), which might get to be placed alongside of the primary setæ and form new primary setæ.

It appears to me just as legitimate to suppose a process of reduction from a more generalised condition, and there is strong evidence that this *has* taken place in *Urochæta*, and that the resemblance between the more common disposition of the setæ and the parapodia of the Polychætous worm is due to adaptive rather than to any genetic causes.

Such an hypothesis would fit in very well with the new facts recorded in the present paper, for I have been able to show that there is no connexion between the *pair* of setæ† and the nephridium, as between the parapodium and nephridium in the Polychæta, and that therefore, in so far as these facts have weight, the dorsal and ventral pairs of setæ do not correspond to the parapodia.

XV. "The Vortex Ring Theory of Gases. On the Law of the Distribution of Energy among the Molecules." By Professor J. J. Thomson, B.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. Received June 4, 1885.

[Publication deferred.]

* "Monographie der Enchytræiden," p. 21.

† It is worth recording the fact that in a species of *Acanthodrilus*, also from New Zealand, where the setæ are paired the nephridial orifice is not placed indefinitely in front of the pair, but has a distinct relation to *one* of the two setæ.

XVI. "Contributions to the History of the Pleiocene and Pleistocene Deer. Part II. *Cervus Dawkinsi* (Newton) and *Alces latifrons* (Dawkins)." By W. BOYD DAWKINS, M.A., F.R.S., F.G.S., Professor of Geology and Palæontology in the Victoria University, Owens College, Manchester. Received June 17, 1885.

[Publication deferred.]

XVII. "On Certain Definite Integrals. No. 13." By W. H. L. RUSSELL, A.B., F.R.S. Received June 18, 1885.

[Publication deferred.]

XVIII. "On Certain Definite Integrals. No. 14." By W. H. L. RUSSELL, A.B., F.R.S. Received June 18, 1885.

[Publication deferred.]

XIX. "The History of the Kew Observatory." By R. H. SCOTT, M.A., F.R.S. Received June 18, 1885.

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"The Development of the Arteries of the Abdomen and their Relation to the Peritoneum." By C. B. LOCKWOOD. Communicated by W. S. SAVORY, F.R.S.

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Anatomists who have written concerning the arteries of the abdomen and their relations to the peritoneum display considerable divergence of opinion. This want of uniformity may perhaps be due to the fact, that owing to the complications of the serous membrane it is hard to trace the course of the vessels without seriously disturbing their relations. Another and most important reason is, that so far as I am aware, no methodical attempt has been made to elucidate them upon developmental grounds. The importance of such an endeavour, in this case, is made clear by a moment's consideration. If the alimentary tract of one of the lower vertebrates be examined a very simple arrangement may be found; most probably a straight bowel suspended by a simple mesentery. Along the spinal attachment of this serous fold a large blood-vessel extends, and branches descend from it to supply the bowel. When such a scheme as this is compared with that which obtains in the human subject, it is evident that many complicated changes have taken place, and it will be found that the vascular supply of the abdominal organs participates in every change they undergo. In this paper it is proposed to consider the various events which occur, and their effects upon the course of the arteries.

It seems unnecessary to state that so far as alterations in the shape and position of the various portions of the alimentary canal are concerned, there is hardly any difference of opinion; they are well known and have been frequently described. The abdominal blood-vessels and the serous membrane have attracted less attention.

As a preliminary, it may be stated that as the development of the intestines and their appendages, the liver, pancreas, and spleen, has such an important influence upon the arteries, it will be necessary to refer to them at some length.

Owing to the difficulty of obtaining and manipulating human embryos young enough to display the alimentary tract in its earliest stages, I found it convenient to study these by examining embryo rats, mice, chicks, &c. Evidently the truth of observations made in this way requires constant verification by comparison with what can be made out in the human subject.

When embryo chicks are used changes prior to the third day need not be taken into consideration; and although the following facts are all well accepted, perhaps I may be permitted to narrate them, inas-

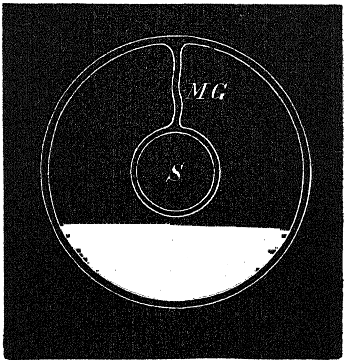
much as they lead up to the points towards which it is desired to draw attention.

A section through the abdomen at about this period (the third or fourth day) displays a body-wall enclosing a pleuro-peritoneal cavity; and in the interior of this an alimentary tube suspended by a mesentery. A more minute examination of the mesentery shows that it is attached to the spine above: that its surface is covered with flattened cells, and that its interior consists of mesoblastic tissue and blood-vessels. The latter spring from a large artery, the aorta, which runs along the base of the serous fold. The vessels of the gut are not merely derived from one or two branches arising from particular parts of the aorta, but are represented by vessels which descend at intervals from the parent trunk. Later it will be evident that only a few of these mesenteric vessels persist, and that the various parts of the alimentary canal retain a supply commensurate with their requirements. Moreover, it will be seen that the development of a great organ, such as the liver, pancreas, or spleen, in intimate relation with the bowel, has an important influence in determining the size of the vessels which pass through the mesentery.

Again, referring to the section through the third or fourth day chick, the following points are easily ascertained. The mesentery and intestine hanging into the pleuro-peritoneal cavity divide it, roughly speaking, into two halves; a right and left (Diagram 1). Of course it follows that the blood-vessels which it contains are bounded on each side by these halves of the peritoneal cavity. As long as this arrangement continues, nothing could be simpler than the relations of the vessels; but complications soon occur, and it will now be attempted to state in detail what they are and how they are produced.

For the sake of clearness, the events which take place in that part of the mid-gut which afterwards becomes the stomach may be con-

DIAGRAM 1.

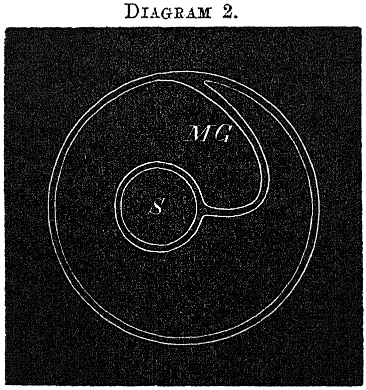


S. Stomach. *MG.* Mesogastrium.

sidered first; although it may be remarked that a great many of the events which are about to be mentioned are in progress at the same time.

The position of the stomach is indicated by an enlargement of the anterior portion of the mid-gut. This organ, like the remainder of the bowel, is fastened to the spine by a mesentery, and simply hangs suspended in the pleuro-peritoneal cavity (Diagram 1). This position is altered by the stomach turning upon its right side. The mesentery, or mesogastrium as it will be called in future, is implicated in this change; instead of descending vertically from the spine to the stomach, it becomes looped (Diagram 2). Owing to these movements of the stomach and mesogastrium the cavity which they partitioned becomes different. There is no longer a right and left division of the peritoneal sac, but a part in front and to the left of the stomach and mesogastrium, and a part behind and to the right. The space which is behind the stomach, and which has the mesogastrium for its left boundary, represents what afterwards is the lesser cavity of the peritoneum. It need not be repeated that blood-vessels pass through the mesogastrium to the stomach, and are of necessity involved in the changes which have just been described. Their course, instead of being straight, forms a curve. The relations of these altered vessels to the subdivisions of the peritoneal cavity require to be noted; it is evident that to reach their destination they now pass round the left boundary of the future lesser sac.

DIAGRAM 2.



S. Stomach. *MG*. Mesogastrium.

All the changes which have been narrated occur at very early stages of development; subsequent events do not produce any great alterations. Later it will be seen that the stomach becomes less vertical, and its mesentery further modified; but the changes which the mesogastrium undergoes are very striking. They have been fully

discussed before,* so that it does not seem necessary to do more than allude to them briefly. Stated as shortly as possible, it may be said that at the œsophageal and pyloric ends of the stomach the mesogastrium remains short, but near the middle elongates and becomes the great omentum. This anomalous structure afterwards enters into relation with the transverse colon; but at present the manner in which this is brought about need not be discussed.

This much having been premised, it may be inquired whether these observations are applicable to the adult subject. The mesogastrium having been identified with the great omentum, it remains to be seen if the blood supply of the stomach still passes through it. The gastric artery certainly conforms to the conditions. This artery begins behind the peritoneum, and runs between two layers of this membrane to reach the œsophageal end of the stomach. These folds of peritoneum represent the upper part of the mesogastrium, and are continuous below with the great omentum. To confirm these statements the lesser sac of the peritoneum should be opened; a hand passed within it towards the right of the œsophagus enters the concavity of a loop which the gastric artery makes; a hand introduced into the greater sac, to the left of the œsophagus, touches the convexity of the loop. When the anatomy of the gastric artery is compared with the original vessels of the mesentery its close resemblance to them is clear; the modifications which have occurred are comparatively insignificant. As it has been explained how the mesogastrium forms the left boundary of the lesser peritoneal cavity, it therefore seems unnecessary to enter into details concerning the relations of the vessels which it contains to the greater and lesser sacs.

If the arteries of the stomach were taken in detail the vasa brevia would come next, and afterwards those near the pylorus. Passing over the vasa brevia, for the moment, the following question seems to demand an answer. If the mesogastrium originally contained numerous vessels, how is it that, with the exception of the vasa brevia, none are found between the œsophagus and pylorus? Generally speaking, structures receive their blood supply by the nearest route, and exceptions to this rule can usually be accounted for. The elongation of the mesogastrium, to form the great omentum, is so excessive that it might almost be anticipated that none of its vessels would persist. Whatever the probabilities may seem, it has always been recognised that the omentum contains enormously long vascular loops, which extend from the curve of the stomach to the transverse colon; they are usually spoken of as being the longest arterial loops in the body. Although these vessels appear to be more for the supply of the

* "On the Development of the Great Omentum and Transverse Meso-colon," By C. B. Lockwood, "Proc. Roy. Soc.," vol. 35, p. 279.

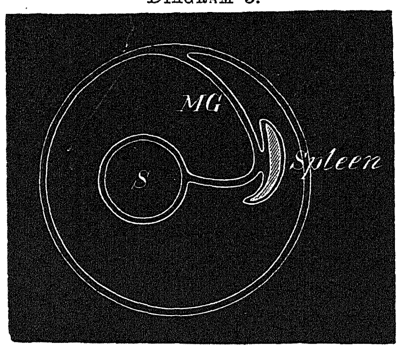
omentum than of the stomach, yet it does not seem unreasonable to argue that they represent the original vessels of the mesentery.

If the course of the gastric artery, through the mesogastrium, has been followed, that of the splenic offers fewer difficulties. Exactly similar principles of development apply to it.

If the mesogastrium of a human embryo about 2 inches long be examined the following facts may be easily observed. Near the œsophagus the mesogastrium is comparatively short, but at the junction of the anterior with the middle third of the stomach it is longer, and the rudiment of a spleen may be distinguished between its layers. This organ is developed from the mesoblast which exists between the layers of the mesogastrium. It will be remembered that a portion of this tissue had previously become differentiated into the blood-vessels of the mesogastrium. Whether the spleen and its blood supply are formed at the same time and from the same tissue, or whether this organ appropriates some of the vessels which the mesogastrium previously contained, would be hard to say. The fact remains that the mesoblast from which this organ is developed is in direct continuity, behind, with that which surrounds the aorta, in front, with that which forms the wall of the stomach; there is no anatomical reason, therefore, why either of the above events might not occur. As soon as the spleen appears blood-vessels can be seen extending to it from the aorta, and onwards to the stomach. Entirely situated between the layers of the mesogastrium the spleen derives the whole of its blood supply from vessels passing through it. At the period at which the spleen is formed the mesogastrium has become involved in the change of position which the stomach undergoes. Owing to the turning of this organ upon its right side, the mesogastrium forms a fold the convexity of which is towards the left. The portion of the general peritoneal cavity included in the concavity of the fold represents the lesser sac. Now the spleen appears at the convexity of the loop, so that the portion of mesogastrium which extends from the spine to the spleen forms the left boundary of the lesser cavity, and that which passes from the spleen to the stomach constitutes the gastrosplenic omentum (Diagram 3). The application of these facts to the course of the splenic artery is not difficult. As this vessel runs in the mesogastrium it ought to commence behind the lesser peritoneal sac, and pass in its left boundary to reach the spleen. It is also clear that a portion of the greater cavity, that which lies between the posterior surface of the mesogastrium and back wall of the abdomen, should be behind the artery. That this is so may easily be ascertained by examining a human fœtus at about the full term. At this period the mesogastrium is well marked, and the splenic artery plainly between its layers. Even in the adult there is a well marked recess, so that, by placing one hand in the lesser sac and another behind the

spleen in the greater, the artery in question may be grasped. Recent works on anatomy (*e.g.*, "Quain's Anatomy," Ninth Edition, 1882, vol. ii, p. 729, fig. 621, &c.) delineate the peritoneum in accordance with the above description. The process of development which has been described seems to afford an explanation of the very complicated relations of this artery, both to the peritoneum and to its greater and lesser cavities.

DIAGRAM 3.



S. Stomach. *MG.* Mesogastrium.

The arteria vasa brevia and gastro-epiploica sinistra are naturally the next to demand attention. It has been pointed out already that the gastro-splenic omentum is simply that part of the mesogastrium which lies between the spleen and the stomach. Blood-vessels are present in the mesentery of the stomach long before the appearance of the spleen, but it does not seem possible to say whether the vasa brevia are the representatives of these, or whether, as was remarked before, they might have been developed in connexion with the spleen itself. Similar doubts appear to exist when the epiploic arteries are in question; how far they are prolongations of the original arteries of the mesogastrium, or to what extent they are developed in the mesoblast which surrounds the stomach. A solution of this point would in no way affect what has been said about the course and relations of the splenic artery itself. This vessel is exceedingly like the gastric, for it has a similar origin and similar course through the mesogastrium.

The arteries in the vicinity of the pylorus are the next to demand notice. The development of the liver and lesser omentum has a profound influence upon these, and therefore requires ample consideration. Owing to difficulties which have been mentioned before their development will be studied in the chick. The very earliest stages of the development of the liver determine its arterial supply. A trans-

verse section through an embryo chick at about the third day of incubation, displays the alimentary canal suspended from the vertebral region by a long mesentery. The liver commences as a protrusion from the wall of the intestine, and owing to the rapidity of its growth the organ soon attains considerable size. Numerous blood-vessels are present in the mesentery which unites the liver and intestine to the spine, and the corpuscles which they contain may be seen to enter into intimate relation with the hypoblastic cells which form the liver substance. This observation is of the greatest importance, for it shows that from its earliest appearance the liver obtains a part of its blood supply from the vessels of the mesentery. Continuing to watch the changes which occur, it will be found that the growth of the liver is very rapid, and that, after a while, it gradually becomes constricted at its junction with the alimentary canal. Whilst this constriction is being formed, the convexity of the growing liver unites with the front wall of the abdomen. The histological changes which accompany these processes are of some importance. The cells which cover the surface of the liver and adjoining parts assume a flattened appearance, and are undistinguishable from those which line the remainder of the pleuro-peritoneal cavity. In other words, the surface of the liver becomes covered with peritoneum. The constriction between the stomach and the liver, at the same time, becomes thinner, and its surface converted into serous membrane. If, at this stage, a recapitulation be permitted, it is evident that the section shows, from the spine forwards, mesentery, intestine, constriction, liver, and attachment of liver to front wall of abdomen. Both Gegenbaur ("Elements of Comparative Anatomy," translated by F. J. Bell, p. 565) and Balfour ("Comparative Embryology," vol. ii, p. 623) point out that this is the usual condition in the vertebrates. The identity of the various parts which have been mentioned has also been recognised by these authors, and seems quite obvious; the mesentery and intestine require no further remark; the constriction between the liver and the bowel corresponds to the lesser omentum, and the part uniting the liver to the front wall of the abdomen is the same as the falciform ligament.

A little consideration will show that although the preceding events have been observed in other vertebrates, yet they are applicable to the human subject. Owing to alterations which take place in the mesentery this is not so easy in the adult, but at about the fifth month of intrauterine life the whole intestine, including the stomach and duodenum, has a well marked mesentery, and it may be seen near the pylorus that the mesoduodenum, duodenum, lesser omentum, liver, and falciform ligament, form an anatomical sequence.

These preliminary facts may now be used in an endeavour to elucidate the course of the hepatic artery.

It has been previously stated that from its earliest appearance the liver derives a portion of its blood supply from the vessels of that part of the alimentary tract from which it grew; and the mesentery was stated to be the route by which it arrives at its destination. Now it is clear that, as development progressed, this supply, besides passing through the mesentery, would have to extend through the constriction which forms between the liver and intestine. In the human subject the duodenum represents that part of the alimentary tract which gives origin to the liver; and, therefore, it seems reasonable to search in its neighbourhood for a vessel which fulfils the conditions which have been laid down. The hepatic artery leaves nothing to be desired; arising behind the peritoneum it runs to the duodenum, and distributes branches to the stomach and intestine; thence it passes onwards through the lesser omentum to the liver, and it is hardly necessary to remark that the lesser omentum corresponds to the constriction which has just been spoken of. One point with regard to the anatomy of the hepatic artery seems to call for explanation and comment; its passage through the mesentery. In the adult the mesoduodenum is often of insignificant size, so that it is not easy to demonstrate the artery passing through it. At about the middle of intrauterine life the mesoduodenum is long, and no such difficulty exists. Perhaps this observation is strengthened by an examination of the course of the vessel in the shrew. In this animal the whole alimentary tract retains the mesentery, and I have satisfied myself that the hepatic artery passes through it before reaching the liver.

If it is true that the hepatic artery is the representative of one of the original vessels of the mesentery, many of its peculiarities may be explained. Instead of one the liver may have two or even three arteries (Cruveilhier, "Traité d'Anatomie Descriptive," vol. iii, p. 67). When this is the case the additional vessels may arise from the aorta and reach the liver in the usual way, or they may take origin from the gastric or superior mesenteric arteries; both of which, it is significant to remark, formerly belonged to the mid-gut. Evidently the organ may appropriate more than one of the original vessels of the mesentery.

That the hepatic artery should distribute branches to the stomach and duodenum need not excite surprise; before the appearance of the liver it performed no other function than to supply that part of the intestine which afterwards becomes converted into those organs.

So far, in order not to introduce needless complications, no reference has been made to the aperture which is called the Foramen of Winslow. The development of the hepatic artery is a factor, but not an essential factor, in its formation. To explain the origin of the Foramen of Winslow, the changes of position which the liver, stomach, and lesser omentum undergo, require to be considered. In

a human embryo, an inch or an inch and a-half long, the stomach is almost vertical. It has been remarked before that the space behind it corresponds to the lesser cavity of the peritoneum. The communication between this and the greater sac lies between the right border of the stomach and lesser omentum and the posterior wall of the abdomen. Now at this early stage of development the liver is of enormous size, and nearly fills the abdominal cavity, descending as low as the pubes. In consequence its transverse fissure occupies an exceedingly low position. Afterwards the relative size of the liver diminishes, and it may be said to retreat beneath the ribs and costal cartilages. It seems reasonable to argue that when the liver makes this ascent it must needs take the lesser omentum, pylorus, and hepatic artery up with it; and it would further follow that the stomach would become more nearly horizontal, and the hepatic artery acquire an upward direction. It is these events which cause the Foramen of Winslow to assume its permanent appearance and position.

Three important arteries, the gastric, splenic, and hepatic, have been passed in review. From the details of their development principles of wide applicability may be deduced.

First. That they were originally derived from the dorsal aorta for the supply of the mid-gut.

Second. That they reach their destination by passing through the mesentery.

Third. That they participate in all the changes the mesentery undergoes.

Fourth. That if an organ is developed in the mesentery or from the gut, it obtains, part at least, of its vascular supply from the vessels of the mesentery, or from those of the gut from which it sprung.

The attempt may now be made to apply these principles to the remainder of the human alimentary canal. Before discussing in detail the arteries of the intestines and their appendage, the pancreas, the development of these organs requires a passing notice. The formation of the large and small intestines has been fully described elsewhere (*e.g.*, Balfour, Gegenbaur, Quain, &c.). Recapitulated as briefly as possible, it may be stated that in human embryos an inch long the alimentary canal forms a loop extending from the stomach to the pelvis. The convexity of this loop is situated in the large aperture which afterwards becomes the umbilicus; its concavity is fastened to the spine by a considerable mesentery. A small protrusion appears about the middle of the intestine, and marks the commencement of the cæcum and vermiform appendix. As the calibre of the upper and lower parts of the bowel

are the same, the outgrowth determines its division into small intestine and colon.

The developmental changes which afterwards occur in the upper part are so slight that they do not seem to demand further comment. An exception may be made in the case of the duodenum, for owing to the presence of the pancreas, the arteries in its neighbourhood are more complicated.

As far as the duodenum itself is concerned, it has been explained why the hepatic artery should send branches to it; the *rationale* of its supply from the superior mesenteric is so obvious that it does not seem to require notice. It would be hard to explain why parts of the duodenum cease to have a mesentery, but the fact of the disappearance of this serous fold can have no influence upon the sources of its blood supply; that was determined long before the mesentery became obliterated.

With regard to the arteries of the pancreas, one or two points await solution. At first it may not be evident why it should be supplied by the vessels of the duodenum and spleen. It is well recognised that the pancreas commences as a cæcal prolongation from the duodenum. In a human embryo about $2\frac{1}{2}$ inches long the elongated mesogastrium, *i.e.*, great omentum, has not yet involved the transverse colon; and when this membrane is lifted upwards, it may be seen continuous with the mesoduodenum. The pancreas, already of considerable size, stretches from the duodenum into the mesoduodenum, and onwards into the mesogastrium,* where it applies itself to the spleen, previously described as being between the layers of this membrane. The splenic artery may be seen as a delicate streak in the mesogastrium close to the pancreas, and is at this time one of its most available sources of blood supply. It is evident that the fact of this organ being at one time between the layers of the mesentery explains why it should be supplied by its vessels, such as the splenic and duodenal; it also explains why, as Haller (quoted by Cruveilhier, vol. iii, p. 68) points out, the pancreas may have a large artery (pancreatica suprema) arising from the cæliac axis, superior mesenteric, or aorta. It will be remembered that the original vessels of the mesentery are numerous, and that even the liver sometimes appropriated more than one; evidently the same is the case with the pancreas. In conclusion, as regards the position of the pancreas between the layers of the mesentery, it is permanent in some animals, *e.g.*, the hedgehog (see also sp. 780A and 781A, R. C. S. Museum). The ultimate relations of this organ to the serous membrane may be conveniently discussed a little further on.

* July 6th, 1885.—This is exactly the anatomical relation which has been described by Professor Anderson as existing in the seal ("Journal of Anatomy and Physiology," xix, p. 228).

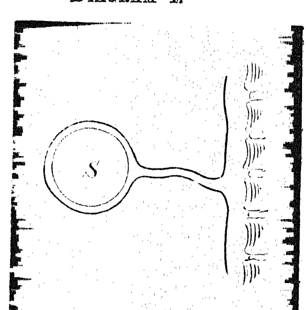
The colon, sigmoid flexure, and rectum are the only parts of the alimentary canal which await examination. When the primitive intestinal loop was mentioned, a part which extended from the pelvis to the cæcal protrusion was left. Keeping in view the fact that the bowel has an abundant mesentery, it remains to be explained how it becomes converted into the ascending, descending, and transverse colons, and how its conversion affects its vascular supply. The simplest way seems to be to trace the movements of the cæcum. At its first appearance this *cul-de-sac* is within the wide umbilical aperture, but as the intestine elongates it performs a tour round the abdomen; from the umbilicus it passes upwards towards the stomach; from thence it journeys into the right hypochondrium, forming the transverse colon, and descending into the right iliac region completes the ascending portion of the gut. When the colon was straight and indistinguishable from the rest of the alimentary tract, its vascular supply consisted simply of vessels which descended along the mesentery; as the bowel travels round the abdomen it carries both mesentery and blood-vessels with it. The peculiarities which are produced in this manner are so slight that they do not seem to demand further notice.

There is, however, an exception in the case of the middle colic artery, for both the anatomy and the development of the mesocolon, in which it lies, are open to discussion. These questions have already been argued at some length by the author in the paper previously alluded to (p. 477), so that in the present instance they may be briefly stated.

As a review of the formation of the omentum and transverse mesocolon naturally leads up to the points at issue, it is convenient to begin by discussing it.

Both the structures in question were mentioned when the development of the stomach and colon was being described (p. 477). It may

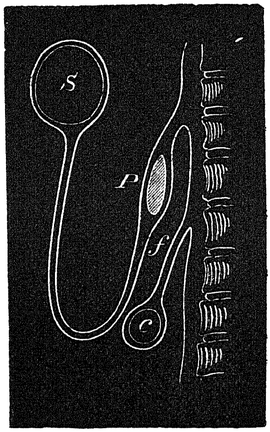
Diagram 4.



S. Stomach fastened to the spine by its mesentery before the latter has become great omentum.

therefore be sufficient to recommence at the point at which their description was discontinued. Beginning with the mesogastrium, perhaps it is remembered that although at first very simple (Diagram 4), yet it afterwards grew and descended from the greater curvature of the stomach to form a loop, the great omentum, and that afterwards it ascended to be fastened to the back of the abdomen. If these ascending layers be traced, the innermost, after reaching the spine, passes upwards to form the back of the lesser sac; the underlayer accompanies it until the spine is reached, and then turns downwards to become continuous with the upper layer of the

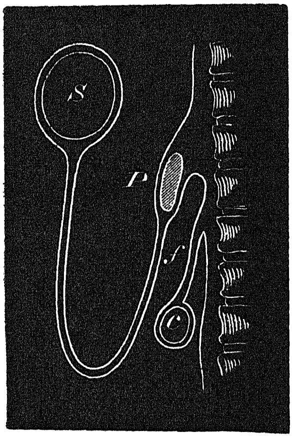
DIAGRAM 5.



S. Stomach. *P.* Pancreas. *f.* Fossa. *c.* Transverse colon.

transverse mesocolon (Diagram 5). Of course it is recognised that at this time the colon has an independent mesentery comparable in every way to that of the small intestine, and it is clear that at this juncture an interval separates the under surface of the omentum from the upper surface of the colon and mesocolon. Now in order to explain how these acquire the intimate relation which they have in the adult, Haller simply stated that they became adherent. This view has been adopted by succeeding authors, and if it is true it must follow, as they say it does, that the transverse mesocolon should consist of four layers of peritoneum, three above the middle colic artery and one below. Although I have examined a great many subjects, the transverse mesocolon has never appeared to consist of more than two layers, one above the artery and one below. If this is true, Haller's theory becomes involved in doubt, even if other objections could not be urged against it. But it seems pertinent to ask why this process of adhesion only occurs in this particular place and

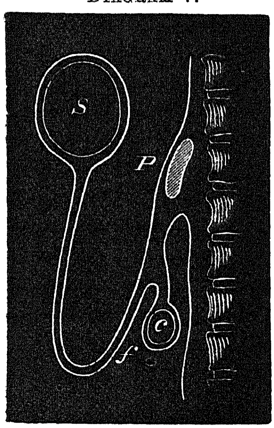
DIAGRAM 6.



The letters are the same as before. The fossa between the omentum and transverse mesocolon is shallower.

nowhere else? Or why, before Haller invented his theory, was the mesocolon always thought to have two layers? The examination of a large number of embryos shows that adhesion never takes place, but that quite a different event happens. Suppose the loop of peritoneum between the under surface of the omentum and the upper surface of the transverse mesocolon was drawn out (see Diagrams 6, 7, and 8, *f*), it would cause the colon to lie between the innermost layer of the omental loop and the inferior layer of the mesocolon; in other words,

DIAGRAM 7.

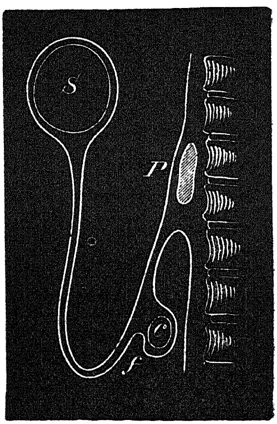


Letters as before. The pancreas is quite behind the peritóneum, the fossa between the mesogastrium and mesocolon having nearly disappeared.

the transverse mesocolon would consist of only two layers, which would embrace the gut and its artery. The paper which has been mentioned was written, and specimens were shown, to demonstrate that this was what took place. If this evidence is true, the usual accounts of the relations of the transverse mesocolon and its contents require to be modified.

It may be remembered that a little while ago it was said that it would be convenient to defer the final stages of the development of the pancreas. When this organ was last spoken of it lay between the folds of the mesogastrium just before this membrane was attached to the spine (Diagram 5). In consequence, the fossa between the meso-

Diagram 8.



The letters as before. To show the last stage in the disappearance of the fossa between the omentum and transverse mesocolon.

gastrium and the mesentery of the transverse colon intervened between it and the spine. It is clear that if the highest part of the peritoneal loop which forms this fossa was withdrawn, the pancreas would naturally apply itself to the spine (Diagrams 6 and 7). That this is what occurs need not be repeated, and as the under surface of the mesogastrium is continuous with the left surface of the mesoduodenum, the drawing out of the one causes the disappearance of the other.

All the arteries of the alimentary canal from the stomach to the rectum have now been discussed, and I have maintained that—

First. All of them, even the splenic and hepatic, were originally derived from the dorsal aorta for the supply of the mid-gut.

Second. That they reach their destinations by passing through the mesentery.

Third. That they participate in all the changes the mesentery undergoes.

Fourth. That if an organ is developed in the mesentery or from the gut, it obtains, part at least, of its vascular supply from the vessels of the mesentery, or from those of that part of the gut from which it sprung.

These principles have, in this paper, only been applied to the development of the blood-vessels of the human alimentary canal, but they seem so simple, and the probability of their truth appears so great, that it may be anticipated that they will be found of very much wider application.

"The Influence of Stress and Strain on the Physical Properties of Matter.* Part I. Moduli of Elasticity—*continued*. Relations between Moduli of Elasticity, Thermal Capacity, and other Physical Constants." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received May 28, 1884. Read June 19.

It has been proved by Wertheim,† whose results have been verified by myself,‡ that if e be taken to denote "Young's Modulus," and α the mean distance between the centres of any two adjacent molecules of a solid body, $e\times\alpha^7$ is, in the case of most metals, approximately a constant. Poisson, regarding a body as an assemblage of molecules very small as compared with the distance between them, bound to one another by an attractive force, and kept at a distance by the repulsion due to heat, was led to the formula—

$$\frac{\mathrm{N}}{\delta}=\frac{2\pi}{3}\sum_{r=\alpha}^{r=\infty}\frac{r^5}{\alpha^5}\frac{d\frac{1}{r}fr}{dr}.$$

In this formula N is a constant force perpendicular to the surface of the body, δ the corresponding linear expansion or contraction produced, r the radius of activity of a molecule, α the mean distance between two adjacent molecules, and fr the function by which the law of the resultant molecular force is expressed. It follows, therefore, that if $e\times\alpha^7$ is a constant, fr must decrease in the inverse

* The original title of the paper has been altered to the above as being more exact in expression.

† "Ann. de Chimie," 1884, tom. xii.

‡ "Phil. Trans.," Part I, 1883, p. 32.

ratio of the fifth power of the distance. Now, Clerk Maxwell in his valuable researches on the viscosity of gases arrived at the important result that the viscosity of a gas is independent of its density, and proportional to the temperature measured from the absolute zero of the air-thermometer.* Maxwell points out that the constancy of the viscosity of a gas for all changes of density when the temperature is constant is a result of the "Dynamical Theory of Gases," whatever hypothesis we adopt as to the mode of action between the molecules when they come near one another; but that if the viscosity be as the first power of the absolute temperature, then in the dynamical theory, which is framed to explain the facts, we must assume that the force between two molecules is proportional inversely to the *fifth power of the distance between them.*† It is true that we cannot regard the distance between two consecutive molecules of a solid as large compared with the diameter of the molecules; indeed, we must, on the contrary, assume the molecules to be nearly touching each other; consequently Poisson's formula would require modification before it could be strictly adapted to the case in point. It is equally true that the equation $e \times \alpha^7 =$ a constant cannot possibly hold good for all temperatures, inasmuch as the value of e decreases more with rise of temperature than α^7 increases, and that consequently the function fr must contain the temperature. Nevertheless, the coincidence of the results as regards the law of force between two molecules obtained by Wertheim and Maxwell in quite different ways, seemed to warrant me in making more extended inquiries in this direction, and with this object I set about making a most careful determination of the thermal capacity of each of the different metals which had been used by me in the earlier portion of this inquiry. My object in doing so will be seen from what follows:—

If we denote the atomic mass by A, the density by Δ, the thermal capacity per unit mass by C_m, and the thermal capacity per unit volume by C_v, we have the following relations:—

$$C_m \times A = \text{a constant};$$

$$C_v = \Delta \times C_m;$$

$$e \times \alpha^7 = \text{a constant};$$

$$\alpha \propto \left(\frac{A}{\Delta}\right)^{\frac{1}{3}}.$$

* "Phil. Trans.," 1866, vol. 126, Part I.

† I find that Maxwell in a later memoir ("Phil. Trans.," 1879, Part I) expresses an opinion that the balance of evidence is against this law. On the contrary, the results obtained by Heen ("Bulletins de l'Académie Royale de Belgique," 3e Série, t. iv, 1882), seem to prove most conclusively that for liquids and solids of a *definite constitution*, as shown by their thermal expansibility at different temperatures, the

From these relations we obtain—

$$\frac{e}{C_v^{\frac{7}{3}}}=\text{a constant},$$

or that the cube of "Young's Modulus" varies as the seventh power of the thermal capacity per unit volume.

Description of Apparatus and Mode of Experimenting.

Thermometers.—Three thermometers were employed; these were made by Casella, and after having been filled were kept by myself for a year; they were then sent back to the maker to be graduated, and a month afterwards their graduations were tested at Kew, and were found to accord very accurately with the Kew standards throughout the whole of their ranges, which were in the three instruments from −5° C. to 30° C., from 30° C. to 65° C., and from 65° C. to 105° C. respectively. The graduations were made to one-tenth of a degree centigrade, and were of such a length that it was easy to estimate to one-hundredth of a degree.

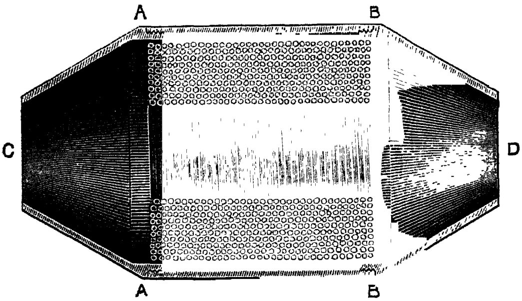
Calorimeter.—The calorimeter was of the well-known kind introduced by Regnault, and the silver coatings of both the inner and outer copper vessels of the instrument, which was new, were throughout the experiments kept uniformly bright. The mass of the inner vessel of the calorimeter was 133 grams, and the water equivalent of it, the stirrer and thermometer, was determined from the mean of several experiments made by introducing into the vessel known masses of water at different temperatures to be 138 grams, whilst the water equivalent, as calculated from the mass and thermal capacity of the vessel stirrer and thermometer, was, on the assumption of the heat being absorbed uniformly throughout the vessel, 140 grams. The probable error of an observation of the thermal capacity of a wire resulting from error in the determination of the water equivalent would in no case exceed 0·06 per cent.

Mode of Adjusting the Wires.

The wires, which had been previously well annealed, were wound round a steel rod, and so made into coils of length about 2 inches, inner diameter $\frac{3}{4}$ inch, and outer diameter $1\frac{3}{4}$ inches; the rod was then withdrawn, and the coil inserted into a thin brass envelope. The envelope (figs. 1, 2) consisted of a hollow cylinder AB, 2 inches in length and 2 inches in diameter, terminated at both ends by a truncated cone. The closed conical end BDB could be unscrewed so as to receive the coil of wire, and was, after the insertion of the coil,

force does vary inversely as the fifth power of the distance. According to M. Heen, the force is inversely as the seventh power of the distance, but it would seem to follow from Poisson's mathematics that the law should be really the one previously indicated.

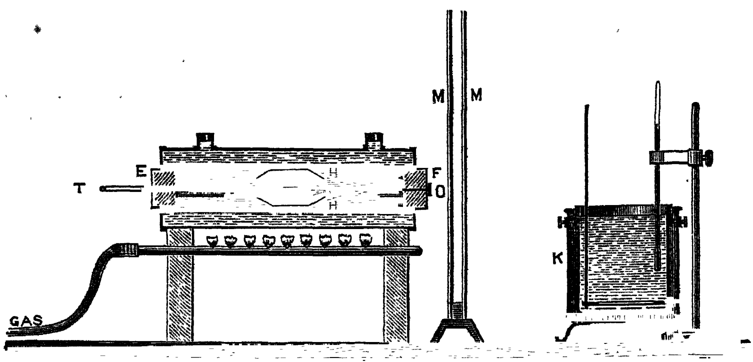
Fig. 1.



screwed on again; whilst at the other extremity of the envelope was an aperture, C, $\frac{3}{4}$ inch in diameter, through which a thermometer could be inserted so that its bulb would lie along the axis and in the centre of the coil of wire.

The envelope served a double purpose, as it not only enabled compensation to be made in the manner presently to be described for loss of heat during the transference of the wire from the air-chamber to the calorimeter, but also was of use in distributing the heat uniformly throughout the coil, which was not the case when no such precaution

Fig. 2.



was taken.* The thermometer was placed in the air-chamber in a position which was nearly horizontal, but slightly slanting upwards so as to prevent the column of mercury in the thermometer tube from being broken when the temperature was falling. As soon as the coil of wire had attained the desired temperature, the calorimeter was

* In some preliminary experiments made without the envelope I frequently found a difference of quite 1° between the temperature of the centre of the coil and that of one end, but with the envelope there was, after a certain interval of heating, no sensible difference of temperature.

placed close to the end F of the air-chamber, and the double screen MM having been first removed the cork F was pulled out, and after it, through the intervention of the thread, the brass envelope with the contained wire, which were speedily dropped into the calorimeter. After the water in the calorimeter and its contents had been stirred for three minutes, the temperature of the mixture was determined to within $\frac{1}{100}$° C. by means of the first of the previously-mentioned thermometers, care having been taken by means of the erection of certain marks to avoid parallax in the reading. After the first reading had been taken two others ensued after successive intervals of three minutes each, so that correction might be made for loss of heat resulting from radiation; this loss, however, being almost compensated by placing in the calorimeter water at a temperature which previous rough calculation had shown would be as much below the temperature of the room as the temperature of the mixture would be above it.

Preliminary observations were made for the purpose of ascertaining the rise of temperature which would be caused by immersing the heated envelope only in the 450 grams of distilled water used in the calorimeter, and the mean of eighteen observations in which the envelope alone was raised to 60° C. and then plunged into water at about 20° C., showed that the average rise of temperature per degree of fall of temperature of the brass envelope between 60° C. and 20° C. was 0·01150° C. Similarly eighteen observations between 100° C. and 20° C. showed a rise of 0·01114° C. to be produced per degree of fall of temperature of the envelope. If the thermal capacity of the brass of which the envelope was formed be assumed to be 0·0924 between 20° C. and 60° C.,* the calculated rise of temperature, supposing that no heat is lost in the transference of the envelope from the air-chamber to the calorimeter, would be 0·0137; so that, as might be expected from the large surface of the envelope as compared with its mass, the loss of heat during the transference is very appreciable, and, as also might be expected, the loss between 100° C. and 20° C. is greater than that between 60° C. and 20° C. It will be sufficient, perhaps, to record the eighteen observations between 20° C. and 100° C.

* My subsequent experiments would prove that this would be very nearly the case.

Experiment I.—The brass envelope heated to 100° and afterwards plunged into 450 grams of water contained in the calorimeter.

Number of observation.	Average rise of temperature of the water and calorimeter per degree of fall of temperature of the envelope.	Departure from the mean value.
1	·01138	+ ·000024
2	·01115	+ ·000001
3	·01106	− ·000008
4	·01121	+ ·000007
5	·01103	− ·000011
6	·01121	+ ·000007
7	·01102	− ·000012
8	·01089	− ·000025
9	·01125	+ ·000011
10	·01136	+ ·000022
11	·01091	− ·000023
12	·01130	+ ·000016
13	·01141	+ ·000027
14	·01096	− ·000018
15	·01115	+ ·000001
16	·01112	− ·000002
17	·01105	− ·000009
18	·01107	− ·000007
Mean	·01114	

The probable error of the mean value is not more than 0·22 per cent., and the probable error of the mean value of the other set of eighteen observations was not more than 0·16 per cent. Experiment II furnishes a fair sample of the accuracy of the work.

Experiment II.—German-silver (1)* well annealed, weighing 121·08 grams.

No. of Observation.	Temperature of the envelope and contained wire before mixing.	Temperature of mixture uncorrected.	Correction to be applied for effects of radiation.	Temperature of mixture corrected.	Rise of temperature caused in the water, calorimeter, &c., by envelope and contained wire.	Calculated rise of temperature caused by the envelope only.	Rise of temperature caused by the wire only.	Mean thermal capacity between 20° C. and 60° C. The thermal capacity of water at 20° C.=1.	Departure from the mean value of all the observations.
1.........	60·60	19·820	−† ·0225	19·7975	1·2875	·4686	·8189	·09366	− ·00058
2.........	59·43	20·205	+ ·0375	20·2425	1·2425	·4507	·7918	·09416	− ·00008
3.........	61·93	22·195	+ ·0375	22·2325	1·2725	·4566	·8159	·09578	+ ·00154
4.........	61·58	21·605	·0000	21·605	1·2850	·4597	·8253	·09624	+ ·00200
5.......	61·70	21·845	+ ·0150	21·8600	1·2600	·4582	·8018	·09380	− ·00044
6...... ...	60·88	20·505	− ·0075	20·5125	1·2775	·4644	·8131	·09385	− ·00039
7.........	56·62	20·140	+ ·0400	20·1800	1·1400	·4191	·7209	·09220	− ·00204

* A number attached to the name of a metal refers to a wire with a corresponding number in a portion of the paper already published ("Phil. Trans.," Part I, 1883).

† A minus sign in the column shows that the water in the calorimeter had been cooled down a little too much before the mixing took place, and that hence the water gained in temperature by radiation.

The mean value of all the observations on the mean thermal capacity of German-silver between 20° C. and 60° C. is 0·09424, but this number requires to be slightly corrected if we are to assume the thermal capacity of water at 0° C. to be unity. According to Regnault,* the thermal capacity of water at $t°$ C. $= 1 + 0·00004t + 0·0000009t^2$, and hence the thermal capacity at 20° C. $=$ 1·00116. Again the apparent weight of water in air was taken instead of the weight *in vacuo*. Now the correction of the apparent weight W of a mass of water when weighed with brass weights amounts to $W \,.\, 0·0012 \left(1 - \frac{1}{8·4}\right) = W \,.\, 0·00106$. On the whole, therefore, we must take $0·09424 \times 1·00222$, *i.e.*, 0·09445, as the corrected thermal capacity. A similar correction was applied in the case of all the other observations. No correction was made in consideration of the fact that the whole of the stem of the thermometer employed in the calorimeter was not immersed in the source of heat, as according to Kopp, if N be the number of degrees extant from the source of heat, T the observed temperature, and t the atmospheric temperature, α the correction to be applied,

$$\alpha = N(T-t) \times ·0001545.\dagger$$

Now if we assume the temperature of the air to be 20° C., and the temperature of the water before and after the immersion of the heated wire to be 18° C. and 22° C. respectively (this representing rather an extreme case), the correction would only introduce an alteration in the value of the thermal capacity of 0·015 per cent. With the thermometer used in the heated air-chamber a correction, not however exceeding in any case 0·3 per cent., was, when necessary, applied by the aid of Kopp's formula.

In Table I are given the results obtained with the different metals, and in Table II a comparison of these results with those obtained by other experimenters.

* "Mém. Acad. Sciences," xxi, p. 729.

† Recent investigations have shown that this formula can only be regarded as roughly approximate, but the approximation is quite sufficient for the present purpose.

Table I.

Name of metal.	Mean thermal capacity per unit mass between 20° C. and 60° C. Thermal capacity of water at 0° C. = 1.	Number of observations between 20° C. and 60° C.	Probable error per cent.	Mean thermal capacity per unit mass between 20° C. and 100° C. Thermal capacity of water at 0° C. = 1.	Number of observations between 20° C. and 100° C.	Probable error per cent.	Formulæ for the number of thermal units required to raise the temperature of unit mass from 0° C. to t° C.	Formulæ for the thermal capacity per unit mass at the temperature of t° C.
Aluminium	·21620	10	0·56	·22080	8	0·34	$\cdot 20700t + \cdot 0001152t^2$	$\cdot 20700 + \cdot 0002304t$
Iron.................	·11162	5	0·81	·11442	5	0·61	$\cdot 10601t + \cdot 0000701t^2$	$\cdot 10601 + \cdot 0001402t$
German-silver	·09445	7	0·48	·09477	3	0·19	$\cdot 09413t + \cdot 0000053t^2$	$\cdot 09413 + \cdot 0000106t$
Zinc	·09308	6	1·20	·09457	3	0·13	$\cdot 09009t + \cdot 0000374t^2$	$\cdot 09009 + \cdot 0000748t$
Copper	·09267	5	0·24	·09396	4	0·35	$\cdot 09008t + \cdot 0000475t^2$	$\cdot 09008 + \cdot 0000648t$
Silver...............	·05640	3	0·38	·05727	3	0·83	$\cdot 05466t + \cdot 0000218t^2$	$\cdot 05466 + \cdot 0000436t$
Tin..................	·05518	5	0·96	·05662	6	0·46	$\cdot 05231t + \cdot 0000361t^2$	$\cdot 05231 + \cdot 0000722t$
Platinum-silver	·04836	11	0·87	·04891	6	0·57	$\cdot 04726t + \cdot 0000138t^2$	$\cdot 04726 + \cdot 0000276t$
Platinum	·03248	5	0·69	·03273	2	0·39	$\cdot 03198t + \cdot 0000063t^2$	$\cdot 03198 + \cdot 0000125t$
Lead	·03120	3	0·74	·03181	6	1·00	$\cdot 02998t + \cdot 0000153t^2$	$\cdot 02998 - \cdot 0000306t$

Table II.

Observer.	Metal.	Formulæ for the thermal capacity per unit mass at $t°$ C.	Mean thermal capacity per unit mass between 0° C. and 100° C.	Departure per cent. from the mean of the values obtained by other observers.
Tomlinson.....	Aluminium	·20700 + ·0002304t	·2185	
Bède.........	Iron	·1040 + ·000144t	·1112 Mean. ·1116	1·4 +
Regnault		.. ..	·1138	
Dulong & Petit		.. ..	·1098	
Tomlinson.....		·10601 + ·000140t	·1130	
Tomlinson.....	German-silver	·09413 + ·0000106t	·09466	
Bède.........	Zinc	·08595 + ·000084t	·09015 Mean. ·09280	1·1 +
Regnault		.. ..	·09555	
Dulong & Petit		.. ..	·09270	
Tomlinson.....		·09009 + ·000075t	·09383	
Bède.........	Copper	·0892 + ·000065t	·09250 Mean. ·09418	0·9 −
Regnault		.. ..	·09515	
Dulong & Petit		.. ..	·09490	
Tomlinson.....		·09008 + ·000032t	·09332	
Dulong & Petit	Silver	·0530 + ·000027t	·05570 Mean. ·05685	0·0
Regnault		.. ..	·05701	
Tomlinson.....		·05466 + ·000044t	·05684	
Bède.........	Tin	·0512 + ·000063t	·05440 Mean. ·05531	1·1 +
Regnault		.. ..	·05623	
Tomlinson.....		·05231 + ·000072t	·05591	
Tomlinson.....	Platinum-silver	·04726 + ·000028t	·04864	
Violle	Platinum	·0317 + ·000012t	·03230 Mean. ·03274	0·4 −
Regnault		.. ..	·03243	
Pouillet.......		.. ..	·03350	
Dulong & Petit		.. ..	·03550*	
Tomlinson.....		·03198 + ·000013t	·03262	
Bède.........	Lead	·0283 + ·000036t	·03010 Mean. ·03075	2·5 +
Regnault		.. ..	·03140	
Tomlinson.....		·02998 + ·000031t	·03151	

* I have rejected this number from the mean, as it is considerably in excess of those of the other observers, and is, moreover, exactly equal to that obtained by Dulong and Petit between 0° C. and 300° C., a result not in accordance with the experiments of Pouillet, Violle, or myself, who have all found the thermal capacity of platinum to rise with the temperature.

Remarks on Tables I and II.

From the last column of Table I we learn that the thermal capacity of all the metals examined increases with the temperature, a result which we find confirmed by the observations of other experimenters recorded in the third column of Table II.*

The last column of Table II shows that the results obtained by me are on the whole slightly, but decidedly, *greater* than those of the means of the other experimenters, and perhaps this *may* be accounted for by the fact that with me the loss of heat in the transfer from the hot air-chamber to the cold water is *entirely* compensated.

The thermal capacities of the alloys platinum-silver and German-silver are almost exactly the same as those calculated from the proportions of their components; thus the calculated thermal capacity of the platinum-silver, which was composed of 2 parts by weight of silver and 1 part of platinum, is 0·04877 between 0° C. and 100° C., and the observed value is 0·04864. Again, the calculated thermal capacity of the German-silver, which was composed of 74 parts by weight of copper, 20 parts by weight of zinc, and 6 parts of nickel,† is 0·09460, and the observed value 0·09466. Such results as these are highly satisfactory, as they not only serve as indications of the trustworthiness of the mode of observation, but also show how very closely we may rely on calculating the thermal capacities of such alloys as these from the thermal capacities of their component parts.

Finally, we learn from Table II how well the observations of different observers agree with each other, so that small differences in the purity of the metal‡ do not, as in the case of electrical conductivity, make much difference in the thermal capacity.

Relation between "Young's Modulus" and Thermal Capacity per Unit Volume.

It is now time to consider whether such a relationship as has been mentioned in the beginning of this portion of the paper to be likely to exist between thermal capacity and elasticity, does really do so, and in Table III will be found the necessary data.

From Table III we learn that, with exceptions in the cases of platinum and platinum-silver, the order of the thermal capacities per unit volume of the different metals is the same as the order of

* The numbers given in this column are taken from Everett's "Units and Physical Constants," p. 83, with the exception of those taken from Table I.

† The component parts of this alloy were determined for me in the Chemical Laboratory, at King's College, through the courtesy of Mr. J. M. Thomson.

‡ The metals aluminium, copper, platinum, and silver were sold to me by Johnson, Matthey, and Co. as chemically pure, whilst the zinc, lead, and tin were as pure as could be obtained by the ordinary process of distillation.

Table III.

Metal.	Density at 20° C. Density of water at 4° C. = 1.	Thermal capacity per unit volume at 20° C. Thermal capacity of water at 0° C. = 1. C_v.	$\sqrt[3]{C_v} = \frac{1}{\alpha}$ where α is proportional to the mean distance between the centres of consecutive molecules.	"Young's modulus" in grams per sq. cm. e.	$\frac{e}{C_v^{\frac{7}{3}}}$. $= e \times \alpha^7$.
Iron (1)	7·759	·8443	·9452	1981×10^6	2941×10^6
Copper (2)....	8·851	·8087	·9315	1218	2001
German-silver (2)	8·632	·8010	·9289	1230	2060
Platinum (1)..	21·309	·6869	·8824	1467	3521
Zinc (1)......	7·138	·6537	·8680	766·9	2067
Platinum-silver (1).....	12·616	·6031	·8149	1044	3397
Silver (1)	10·464	·5811	·8344	753·4	2674
Aluminium (1)	2·731	·5780	·8329	671·2	2414
Tin (1)	7·264	·3904	·7308	277·1	2489
Lead (1)	11·193	·3424	·6995	167·0	2038
				Mean....	2561×10^6

"Young's Modulus," whilst it will be seen from the last column of the table that the products of e and $\frac{1}{C_v^{\frac{7}{3}}}$ all lie between 20×10^8 and 30×10^8, except as before in the case of platinum and platinum-silver. It seemed advisable therefore to ascertain how far the constancy holds good in the case of such other metals as have been examined by other observers, for the purpose of determining the values of "Young's Modulus" and the thermal capacities per unit volume. Table III is accordingly supplemented by Table IV.

Table IV.

Metal.*	Density.	Thermal capacity per unit volume between 0° C. and 100° C. C_v†.	"Young's modulus." e.	$\frac{e}{C_v^{\frac{1}{3}}}$.
Cadmium .	8·665	·4913	575×10^6	3017×10^6
Glass	2·942	·5207	614·4	2819
Gold	18 275	·5920	685·5	2330
Brass	8·471	·7958	1005	1714
Palladium.	11·288	·6694	1077	2747
Piano-steel.	7 727	·8748	2049	2801
Nickel	8·705	·9559	2480	2755
			Mean	2597×10^6

Of the seven substances entered in Table IV, we find five for which the values of $\frac{e}{C_v^{\frac{1}{3}}}$ lie closely to the mean value 2597×10^6, whilst brass, an alloy be it remembered of copper and zinc, furnishes an exceptionally low value. The mean value here agrees fairly with the mean value recorded in Table III, namely, 2561×10^6. But even in some of those cases where the departure from the mean value is great, more recent investigations of my own on "Young's Modulus" and on torsional rigidity, in which very great precautions have been taken to avoid the effects of imperfect elasticity, have resulted in giving values of e which would make the products of e and $\frac{1}{C_v^{\frac{1}{3}}}$ lie closer to the mean values here obtained. This is so for the metals copper, zinc, and brass, but not for platinum, which is still remarkable for having the value of $\frac{e}{C_v^{\frac{1}{3}}}$ much higher than the mean. It may be remarked that the ratio of lateral contraction to longitudinal extension was, according to my previous determinations, *much less* with platinum than with any of the other metals.‡

* The numbers in columns 1 and 4 are, with an exception in the case of nickel, taken from Sir W. Thomson's article on Electricity, "Encyc. Brit." For the elasticities of nickel see "Phil. Trans," Part I, 1883, p. 126.

† As there are no data for determining the thermal capacity at 20° C., that between 0° C. and 100° C. is here given: therefore C_v is here slightly higher than it should be if we are to compare the results given in the two tables.

‡ *Loc. cit.*, p. 28.

OBITUARY NOTICES OF FELLOWS DECEASED.

GEORGE BENTHAM was born on the 22nd of September, 1800, at Stoke, near Portsmouth, where his parents happened to be at that time residing, in consequence of the professional occupations of his father, General, afterwards Sir Samuel, Bentham, who then held the important post of Inspector of Naval Works under the Admiralty. His mother, a lady of great ability, was the daughter of Dr. George Fordyce, F.R.S. He was the second son and third child of a family of five, all of whom he survived.

The conditions of young Bentham's early days were hardly favourable to the settled routine of an orthodox scheme of education; for he was trained by private tutors, and never went to school nor college. But although it was a life-long regret to him that he had not been subjected to the associations and discipline of school and college—to which deprivation, no doubt, a certain shyness and reserve that characterised him may be attributed—his was not a mind to run to waste even under the unsettlement of strong contrasts of life and circumstances occasioned by the migration of his father and family to Russia, and afterwards to the South of France, where the administration and control of a large estate belonging to the father were confided to the junior in his early manhood.

As a youth he was an eager student in many branches of knowledge, with a special aptitude however for the acquisition of languages: while yet a lad of six or seven years he was able to converse fluently in French, German, and Russian. Methodical habits and a capacity for close and prolonged application to any favourite study appear to have been ingrained in him from very early days, and, with his retentive memory, laid the sure foundation of his future eminence in the field of Systematic Botany. He was first attracted, as is assumed, to this branch of science by the "Flore Française" of De Candolle, the analytical tables of which, as an aid in the determination of species, appear to have had a special fascination for him—falling in with the "methodising, analysing, and tabulating ideas" which he had derived from previous study of the works of his eminent uncle Jeremy Bentham. The study of other works of De Candolle's on the structure and classification of plants, and the personal friendship and influence of that eminent botanist, strengthened Bentham's bias towards Classificatory Botany. This indeed may, from about

his twenty-fourth year onward, be regarded as his serious life's work, interrupted more or less, however, for some years by legal research with a view to a professional career, and by the services which he rendered in the preparation of his uncle's works for the press.

Mr. Bentham's first botanical work, published in 1826, was his "Catalogue des Plantes Indigènes des Pyrénées et du Bas Languédoc," the result of a careful botanical exploration of the Pyrenees, in company with the late Dr. Arnott (afterwards Professor of Botany in the University of Glasgow). To appreciate rightly this little work of 128 pages, we must compare the botanical science of that date with our present detailed knowledge of Pyrenean Botany, and of the relation of the vegetation of this region to that of other European chains. At that time there was no accurate scientific account of the Botany of the Pyrenees, and the loose practice of the authors of local floras freely allowed, on the most slender indirect evidence, admission into their enumerations of very unlikely species. But Mr. Bentham says, "Pour donner à mon catalogue une utilité plus générale que celle des simples compilations, si faciles à faire et par conséquent si multipliées malgré leur peu d'utilité, je me suis attaché à n'y admettre aucune observation, aucun synonyme que je n'ai pas eu occasion de vérifier par moi-même, ou, si je me suis écarté de cette règle, cela n'a été que très-rarement, et toujours en citant la personne de qui je les tiens." This habit of carefully scrutinizing his data he maintained to the end of his life. Besides an enumeration of Pyrenean plants this catalogue includes critical essays on several peculiarly difficult genera, especially *Cerastium*, *Helianthemum*, *Linum*, *Medicago*, *Myosotis*, and *Orobanche*.

Mr. Bentham's botanical life in England may be regarded as beginning with his association with the late Dr. Wallich in the distribution of the enormous Indian collections of that naturalist, and with his elaboration of the great natural order Labiatæ, and of the Indian Scrophulariaceæ: orders which some years afterwards he revised for the "Prodromus Systematis Naturalis Regni Vegetabilis" of his friend De Candolle. It is in these works that we may first recognise the peculiar qualities which give value to all Mr. Bentham's taxonomic work. They show an insight, of so special a character as to deserve the name of genius, into the relative value of characters for practical systematic work, and, as a consequence of this, a sure sifting of essentials from non-essentials in each respective grade. At the date of these works the broad foundations in classificatory Phanerogamic botany had, we must remember, been already securely laid; and the pressing need, supplied so precisely at the right time by Mr. Bentham, was for a systematist capable of reducing to order the number of genera which in some of the largest natural orders were still simply an unorganised mob.

Mr. Bentham published his "Labiatarum Genera et Species" in 1832-36: his "Scrophularineæ Indicæ" in 1835.

In 1829 Mr. Bentham accepted the onerous duties of Honorary Secretary to the Horticultural Society of London. The Society was, at that time, in very low water; but with his friend Dr. Lindley's assistance he rescued and brought it into "a flourishing condition, financially and scientifically, which it has never since approached." This connexion with the Horticultural Society necessarily determined somewhat the direction of his botanical work, and led him to the publication of the botanical results of the Douglas and Hartweg explorations in North and Central America and in the United States of Columbia.

In 1833 Mr. Bentham married the daughter of the late Right Hon. Sir Harford Brydges, of Boultibrooke, and settled in the following year at his late uncle's house in Queen's Square Place, whence he removed in 1842 to Pontrilas House, in Herefordshire. It was while resident here that he revised the orders Labiatæ, Scrophulariaceæ, and the Eriogoneæ for the "Prodromus," besides continuing his publication of the Guiana plants of Schomburgk, and the botanical collections of the voyage of the "Sulphur" (1844).

In 1836 Mr. Bentham made a tour with the object of visiting the principal herbaria of the Continent, and settled to hard work in Vienna in the autumn of that year. It was here that he prepared his first important work on the Leguminosæ ("Leguminosarum Generibus Commentationes"), published in 1840 in the "Annalen des Wiener Museums." He also described, in conjunction with Dr. Endlicher, the Australian novelties brought home by Baron Hügel, and worked at the Ericeæ for the "Prodromus."

In 1854, finding the maintenance of his herbarium and library too serious a charge, Mr. Bentham contemplated, on his final removal to London, abandoning botanical work and presenting his collections and books to the Royal Gardens, Kew, of which his old and most intimate friend Sir W. J. Hooker was Director. That eminent man, however, by his entreaties and by the offer of the free use of his library and herbarium, prevailed to secure to Systematic Botany the rest of Mr. Bentham's active life. Through his influence the accommodation of a private working room in connexion with the herbaria at Kew was secured for Mr. Bentham, and the course of his future life's work was finally determined by the inauguration of a series of Colonial Floras, of the model of which, the "Flora of the Island of Hong Kong"—so remarkable in the number of forms peculiar, or at that time supposed so to be, to that small island—Mr. Bentham was the author. The "Flora Hongkongensis" was published in 1861. A short time before Mr. Bentham had published, for the use of beginners, an admirable flora of the British Islands, a work in

which the species are described in simple but accurate English, and which possesses an excellent analytical *Clavis*. Here the writer of this short notice may remark upon the perfection of Mr. Bentham's descriptive work, manifest not only in its terseness, aptness, and precision, but especially in the judicious selection of diagnostic marks, and in the instinctive estimate of probable range in variation which long experience and innate genius for such work could alone inspire. In 1863 Mr. Bentham published the first volume of the greatest work of the colonial series yet given to the world, the "Flora Australiensis," based upon the collections at Kew and Melbourne, the latter annotated and transmitted with noble liberality by our Fellow Sir F. Mueller. This work was completed in seven volumes in 1870.

In 1863 Mr. Bentham accepted the Presidency of the Linnean Society, which he retained until 1874, working with head and hand as no President ever did before for the welfare of any learned society. His anniversary addresses are, in their way, masterpieces, embracing wide fields and discussing biological questions with a liberality and breadth of view surprising even to his nearest friends.

While the Australian flora was yet in progress Mr. Bentham, in conjunction with his friend Sir Joseph Hooker, entered upon the crowning work of his life, for which his long experience in systematic work had pre-eminently fitted him, the "Genera Plantarum." This occupied him, with comparatively slight interruption by such serious trifles as the elaboration of the Leguminosæ of Brazil for the great Flora of von Martius and the like, until the spring of 1883. No sooner had he completed this great work, the "Genera Plantarum," than it seemed as though he felt that his life's work had been accomplished. His strength rather suddenly gave way, his daily visits to Kew became intermittent, ceasing at last in April, 1884, and gradually becoming feebler he sank to rest, dying simply of old age, with his mental faculties bright nearly to the last, on the 10th September, 1884, within a fortnight of his 84th birthday.

With Mr. Bentham we lose a master in Systematic Botany. The excellent and interesting biographical notice of him in "Nature" by his most intimate friend Sir Joseph Hooker, of which the present writer has made free use, says "there is not a temperate or tropical region of the globe whose floras have not been largely elucidated by him. It may safely be affirmed that for variety and extent of good work of the kind he had no superior. The distinctive qualities of his descriptions are—scientific accuracy, good arrangement, precision of language, lucidity, and the discarding of what is superfluous. In these respects he has had no superior since the days of Linnæus and Robert Brown." And, again, "of his amiable disposition, and his sterling qualities of head and heart, it is impossible to speak too highly; though cold in manner and excessively shy in

disposition, he was the kindest of helpmates and most disinterested of labourers for others." The compiler of this notice, who had the privilege of association with him in the work of the Kew herbarium for twenty-five years, can most feelingly answer to the truth of this.

A Royal Medal was awarded to Mr. Bentham in 1859: he was elected a Fellow of the Royal Society in 1862. The titles of ninety papers by his own hand, and seven written jointly with other botanists, occupy nearly three pages in the Society's Catalogue of Scientific Papers. He was a Companion of the Order of St. Michael and St. George, and member or associate of almost every society in Europe, America, and Australia, which recognises biological studies.

He left no relative except a grand-niece.

D. O.

THOMAS WATSON was born in Devonshire on the 7th March, 1792, as appears from the register of his birth in the parish church of Kentisbeare, in that county. His early education was begun, during the head mastership of Dr. Malken, at the Grammar School of Bury St. Edmund's, where he was a contemporary of the late Bishop Blomfield, with whom he was always on terms of intimate friendship.

On leaving Bury school he entered as a pensioner at St. John's College, Cambridge. He graduated B.A. in 1815, when he was tenth wrangler, and in the following year was elected Fellow of his College, and became M.A. in 1818. According to a rule which then existed those Fellows of St. John's who were not in holy orders, could retain their Fellowships for a short time only. From this rule, however, certain Fellowships, one of which was set apart for the study of medicine, were exempted, and Mr. Watson held his Fellowship until his marriage in 1825, in which year he took his M.D. degree.

While at Cambridge he taught private pupils, and in 1823–24 he served the office of Junior Proctor. During his seven years' residence as Fellow of St. John's he was occasionally absent for months together; in 1819, he was a student of medicine at St. Bartholomew's under Mr. Abernethy, and in the session of 1820–21, he attended the medical classes in Edinburgh. In 1825 he married the daughter of Edward Jones, Esq., of Brackley, Northamptonshire, and commenced practice in Henrietta Street, Cavendish Square. In 1826 he was elected a Fellow of the College of Physicians, and in the following year physician to the Middlesex Hospital. In 1828, when University College was opened, Dr. Watson was appointed Professor of Clinical Medicine, and gave lectures on cases under his care in the wards of the Middlesex Hospital. This appointment he held until 1831, when, at the opening of King's College, he was appointed Professor of Forensic Medicine in that institution.

In 1830 he had the terrible sorrow of losing his wife, who died suddenly three days after the birth of their second child. In 1836 Dr. Watson succeeded the late Dr. Francis Hawkins as Professor of Medicine in King's College, and he retained that chair until the spring of 1840, when at the opening of the newly founded King's College Hospital he was called upon to resign either his office of physician to the Middlesex Hospital or his chair at King's College; and he decided to retain the former office. The resignation of his professorship led at once to a result which proved beneficial alike to Dr. Watson, to the profession, and indirectly to the public, namely, the publication of his admirable lectures "On the Principles and Practice of Physic." The lectures appeared first in the weekly numbers of the "Medical Gazette;" the first lecture was published on 20th September, 1840, and the last of the series on 23rd September, 1842. In the following year, 1843, they were collected and published in two volumes. Between that date and 1871, four large editions were called for. The publication of these lectures, admirable as they were by universal consent acknowledged to be, not less for the soundness and wisdom of their teaching than for their lucid, elegant, and scholarly style, greatly increased the reputation of their author, acquired for him the well merited title of the "Cicero of English Medicine," and led to a large and rapid increase of his practice.

In 1862 Dr. Watson, having held most of the minor offices in the College of Physicians, was elected President, an office to which he was unanimously re-elected for four successive years. The College would have gladly elected him for the sixth time, but he declined on the plea of advancing years, and at the annual meeting for the election of President in 1867 he bade the College farewell in the following characteristically graceful terms:—"It only remains that I should attempt to do that which I feel to be well nigh impossible—to embody in any form of words that I can devise the deep and inextinguishable sense of gratitude with which my mind is full for that kindness and trust which have placed me year by year on five successive occasions at the head of the College of Physicians, in other words, at the head of the medical profession in this great country. According to my estimation more than once expressed, there is no nobler position in medicine, whether I look before me and around me to the body of men from whom it comes, or backwards to the splendid list of names of those who have preceded me in the presidential chair—Linacre, Caius, Glisson, Sir William Browne, Pitcairn, Sir George Baker; these, to go no later, are but a few of the eminent men and sound scholars with whom it may well be deemed a proud distinction to have had one's name in any way associated. But besides this great and repeated honour—the greater because so repeated—I have much else to thank you for. I have to acknowledge your indulgence towards

the many shortcomings of which I am but too conscious. I have to express my thanks for your constant support and counsel in all difficulties, for your unvarying courtesy and deference, for the friendships which my official intercourse with you has formed or strengthened, and most especially for that recent and touching evidence of your approbation and esteem shown by your wish to possess within your walls some pictorial remembrance of my unworthy person. Of this high and generous compliment I can never, while life and reason remain to me, be other than most gratefully, and I hope pardonably, proud. Further, I have to rejoice that the happy lustrum during which I have presided over your affairs has been harmonious and peaceful—disturbed by no unseemly quarrels or serious differences among us—stained by no scandal arising within our proper body, and productive through your exertions and self-sacrifices of something, at least, of benefit to the common weal. If I find anything to regret it is that I have not taken larger advantage of the opportunity which you have confided to me of promoting the interests of the College, and of our useful and noble profession. Still, I must cherish the hope that the College has suffered no abatement of its ancient dignity and renown through my occupation of the office which I now respectfully render back into your hands. And so without encroaching further upon your time, and in redemption of the pledge which I gave you last year, I bid you, as your President, one and all a cordial, affectionate, and final farewell."

The "pictorial remembrance" to which he alludes is an admirable likeness by his old friend, George Richmond, which was subscribed for by the Fellows, and which is now among the most cherished treasures of the College. A replica is in the possession of the present baronet, and the picture has been most successfully engraved by the great and venerable artist Samuel Cousins.

Dr. Watson was appointed Physician Extraordinary to the Queen in 1859, and in 1870 one of the Physicians in Ordinary. On the 9th December, 1861, he was summoned to attend the Prince Consort at Windsor in consultation with Sir James Clark, Sir Henry Holland, and Dr. (now Sir William) Jenner, and his attendance continued until the lamented death of the Prince on the 14th December.

In 1866 Dr. Watson was created a baronet, the honour having been conferred upon him, as the then Prime Minister, Lord John Russell, informed him, by the express desire of Her Majesty.

Among other distinctions which were conferred upon him may be mentioned the following. He was elected an Honorary Fellow of his old College at the same time as the late Sir John Herschel. He was made Hon. D.C.L. Oxford, in 1862; Hon. LL.D. Cambridge, in 1864; and Hon. Fellow of the King's and Queen's College of Physicians, Ireland. In 1859 he was elected a Fellow of the Royal

Society. For many years he was a most active and influential member of the Council of King's College, London. During the session 1857–58 he was President of the Pathological Society. In 1868 he became the first President of the Clinical Society, and in his inaugural address he impressed upon the Society, with his customary good sense and grace of style, the supreme importance of an endeavour to obtain "more exactness of knowledge, and therefore more direct and intelligent purpose, and more successful aim in what is really the end and object of all our labours—the application of remedies for the cure and relief of disease."

During the last ten years of his life he had retired from the active practice of his profession, but he continued to take great interest in all its concerns. His three latest essays on Zymotic Diseases, on Hydrophobia, and on Small-pox and Vaccination, were republished in a small volume in 1879, when he was on the verge of his ninetieth year; yet in these latest products of his pen there is no evidence that age had dimmed his intellect or lessened his command of graceful and expressive language.

After Sir Thomas Watson had retired from the Chair of the College of Physicians, he always attended the annual meeting for the election of President. The last occasion on which he appeared was a few months before his death, at the second election of Sir William Jenner to the Presidency. That meeting was rendered memorable from the circumstance that, in the absence of the Senior Censor, Sir Thomas Watson was called upon, as the senior Fellow present, to deliver to the newly elected President the insignia of his office; and when he got up to walk towards the President's chair the whole of the assembled Fellows rose as one man to show their respect and affection for their venerable ex-President. Notwithstanding his great age he enjoyed his usual good health, spending the greater part of the summer and autumn of 1882 with his daughter and his son's family, partly at the sea-side, and partly at his son's house at Reigate. On Sunday, the 22nd October, soon after attending the morning service at Reigate Church, he was seized with slight paralysis of the left side. He calmly remarked to a medical friend who visited him soon after, "this is the beginning of the end," and so it proved. The paralytic symptoms soon increased and confined him to his bed. He retained his consciousness until within the last two days of his life, though his power of speech had latterly become much impaired. At length, on the 11th December, he sank into a slumber, and so, near midnight, came the final rest for which he had longed and prayed.

Thus passed away one of the wisest and best of men, and one who, by universal consent, was regarded as the most complete illustration of the very highest type of a physician. His own lectures

and public addresses afford the best and fullest illustration of his mind and character; and that he taught no less by example than by precept was known and acknowledged by all who had the privilege of his acquaintance. He was, in fact, a living embodiment of the principles which he so eloquently expounded in his lectures.

One of his most remarkable and admirable characteristics was his freedom from prejudice, and the judicial impartiality with which he weighed and considered any facts or arguments which might be adduced in opposition to principles and doctrines which he had adopted and publicly taught. A writer in the "British Medical Journal" says of him: "The opinions which he formed were always provisional—formed upon the best evidence then available, but subject to revision. The last edition of his celebrated lectures testifies to his rare gift of judicial impartiality, and to the admirable candour and philosophic modesty with which he revised and altered the opinions of earlier years, and the unfaltering courage with which he avowed such changes of opinion. Among the most notable instances of such changes were the new doctrines which he accepted with regard to the theory of the change of type of disease and the pathology of cholera. In both instances he had watched with careful study the progress of medical knowledge, and in neither did he hesitate at the close of the controversies to which they gave rise, to declare himself convinced in a sense contrary to his former opinion, and to set forth with the utmost clearness and graceful simplicity the new conclusions to which he had been led."

We have it on the best authority that when one of his medical friends reproached him for having, in the last edition of his lectures, adopted and given his sanction to so many novel doctrines, his reply was, "Although I am advanced in years" (he was then nearly eighty) "I hope I am not too old to learn;" and so to the very last he continued to take a keen interest in the progress of medical science, and the improvement of medical practice.

G. J.

Robert Alfred Cloyne Godwin-Austen was the eldest son of the late Sir Henry E. Austen, of Shalford House, near Guildford, a gentleman who was for some time an officer in the Household of William IV, and received the honour of knighthood. The subject of this notice was born at his father's house on the 17th March, 1808.

Robert A. C. Austen, as he was then called, was sent to a school situated at Midhurst, in Sussex, which at that time had a very high reputation. This school, which was an ancient foundation, had for its head-master Dr. Bayley, an Oxonian of great classical erudition, who had been an assistant-master at Winchester, and conducted the establishment on the plan of the great public schools. It is a remark-

able circumstance that Charles Lyell had some time before been educated in the same school. If we may judge, however, by the amusing sketch given in Lyell's Autobiography ("Life and Letters," 1881) of the rough and almost brutal system adopted in the school, there can have been nothing in the studies, the associations, or the influences of the school at all calculated to foster that love of natural science which became so conspicuous in the after lives of both Lyell and Austen. After spending some time at Midhurst, Robert Austen went to France, and in a semi-military school there laid the foundation of that knowledge of the French language and its literature which proved so useful to him in his subsequent scientific labours.

The career of young Austen at Oxford, where he was next sent, must have commenced somewhat early, for before he had reached the age of twenty-two he had taken his degree and been elected a Fellow of Oriel College. At Oxford he was, like Lyell, a pupil of Buckland's, and under his persuasive influence imbibed that passion for geological study which henceforth became the distinguishing feature of his life.

Destined by fortune for the life of an English country-gentleman, Robert Austen in 1830 became a student of Lincoln's Inn, and the knowledge of law which he there acquired was doubtless of great service to him during after years in the discharge of his duties as a justice of the peace. But it is clear that at this time Austen's studies were not entirely devoted to the law. At Lincoln's Inn he met Lyell, then just returned from his geological explorations in the south of Europe, and engaged in the completion of the first volume of his "Principles of Geology;" Leonard Horner, then Warden of the London University, and Murchison were also resident in London; and on the 19th of March, 1830, Robert Austen, introduced by these three friends, was elected a Fellow of the Geological Society. At that time Sedgwick was President of the Society, and De la Beche, Whewell, Greenough, and the three friends already mentioned were among the most active Members of its Council. In listening to the papers which described the numerous and important geological discoveries of that period, and to the debates, always animated and instructive, often amusing, and sometimes stormy, which followed the reading of those papers, Robert Austen doubtless increased that knowledge of the infant science, the foundation of which had been so well laid by the teachings of Buckland.

In 1833 Robert Austen was married at Teignmouth to Maria Elizabeth, the only daughter, and afterwards the heiress, of the late Major-General Sir Henry Thomas Godwin, K.C.B. This officer at a later date, namely, between the years 1851 and 1853, commanded the Burmese Field Force during the campaigns which resulted in the addition of the Province of Pegu to our Indian possessions. On the

death of General Sir Henry Godwin, in October, 1884, Robert Austen obtained by Royal licence permission to add the name of Godwin to that of Austen.

In 1834 Austen went to reside at Ogwell House, near Newton Abbot, Devonshire. Here he received frequent visits from Sedgwick and his other geological friends. No better centre for study could have been selected by the young geologist. The richly fossiliferous Devonian limestones, the outliers of Cretaceous strata, and the Tertiary deposits of Bovey Tracy, were all within easy reach, and as proof of the good use he made of his opportunities it may be mentioned that De la Beche entrusted to him the construction of portions of the Devonshire map, while Phillips found in the collection of Ogwell House many of the choicest specimens figured in his "Palæozoic Fossils of Cornwall, Devon, and West Somerset." Between the years 1834 and 1840 a number of valuable papers dealing with the district in the West of England, where he had gone to reside, were read by Robert Austen to the Geological Society, and published in their Proceedings and Transactions.

Returning to his native county in 1838, Robert Austen, after a brief residence at Shalford House, went to live at Gosden House, and subsequently at Merrow House, both situated near Guildford. At a later date, 1846, he removed to Chilworth Manor, in the same county. Here he was within an easy distance from London, and was able to take an active part in the work and management of the Geological Society. Between the years 1841 and 1876 he was frequently a Member of the Council; in 1843–44, and again in 1853–54, he was Secretary, and between 1865 and 1867 he acted as Foreign Secretary of the Society. Although he was frequently nominated Vice-President, it was a subject of regret to all his geological friends that he could not be induced to accept the Presidency. In 1849 he became a Fellow of the Royal Society.

Upon Austen's return to his native county of Surrey, he commenced that series of careful researches on the geology of the South-East of England, the results of which were laid before the Geological Society between the years 1843 and 1853, and did so much to extend our knowledge of the Wealden, the Neocomian, and the Cretaceous systems. During the same decade he spent much time in yachting, always making use of the opportunities afforded for pursuing his favourite studies, and in these he was encouraged and assisted by his friend and frequent companion Edward Forbes. During these excursions he made the observations which were embodied in a series of remarkable and suggestive essays on the valley of the English Channel and the drift of its shores, on the geology of the Channel Islands, the Boulonnais, and other parts of France. Upon Forbes' death in 1854, Austen, acting as his literary executor, completed his two unfinished

works, "The Tertiary Fluvio-Marine Formation of the Isle of Wight," and "The Natural History of the European Seas."

High as the reputation of Godwin-Austen (for so we must henceforth call him) deservedly was as a patient observer, an accurate describer, and a close reasoner, he now began to give proof that his abilities and sympathies were not confined to the narrower sphere in which he had first won distinction. His versatility of mind, his capacity for doing work of a much higher order and for following out the most difficult lines of philosophical investigation, began to be displayed in a series of essays which have justly earned for him the title of the "Physical Geographer of bye-gone periods." The many-sidedness of his mind was exhibited, not only in the circumstance that he was able to complete Forbes' Essay on the Distribution of Marine Forms of Life, but by the fact that in 1840 he read before the Geological Society a remarkable and suggestive palæontological memoir; the views which he at that time enunciated on the zoological position of the extinct forms of Cephalopoda have perhaps not yet received from naturalists the attention which they deserve. At the British-Association meeting at Birmingham in 1849 we find him treating with great ability two difficult questions of Botanical Morphology.

Mr. Godwin-Austen's various studies had led him to consider carefully the conditions under which different geological deposits were formed. Long ago he threw out the suggestion that the Old Red Sandstone and the Poikilitic strata are of lacustrine origin; his interesting essays on the occurrence of blocks of granite and coal embedded in the midst of the chalk exhibit the same prevailing tendency of his speculations. Even before the year 1850 he had undertaken to write the "Geological History of the European Area," and he also contemplated the possibility of preparing an atlas exhibiting the distribution of land and water at different geological periods. It is not difficult to gather from some of his later essays that in the course of time he became convinced of the impossibility of performing such a task in a manner which would have satisfied his accurate and critical mind; but at this time a problem fortunately presented itself to him, which his extensive and minute knowledge of the geology of Southern Britain, Northern France, and Belgium, and his powers of insight and generalisation admirably fitted him to grapple with.

That a ridge of palæozoic, and possibly coal-bearing, rocks extends beneath the Secondary and Tertiary strata lying beneath the Mendip Hills and the Ardennes had been suggested with more or less distinctness by Buckland, Conybeare, and De la Beche in this country, and by De Beaumont, Dufrenoy, and Meugy on the Continent. But in his famous essay "On the Possible Extension of the Coal-measures

beneath the South-Eastern part of England," which was laid before the Geological Society in 1854, Godwin-Austen not only made the subject especially his own, and exhibited his exceptional ability for dealing with geological problems of the greatest intricacy, but he accomplished what was perhaps the highest service to science at that time, by convincing those who had not paid special attention to the subject, that geology was now entitled to take its place in the family of sciences, and was no longer, as the world generally regarded it, a mass of crude theories and baseless speculations. When, in the following year, a deep boring at Kentish Town demonstrated the accuracy of Godwin-Austen's reasonings, and established the truth of his conclusions, it was felt that lustre had been reflected upon the science, no less than upon its able votary.

During his later years, Godwin-Austen was prevented by ill-health from taking so constant and active a part in the management of the Geological Society as formerly. His devotion to science was, however, unabated. Almost every year he accompanied a party of geological friends on some Continental tour; and several of these excursions gave rise to thoughtful and suggestive essays. In 1862 he received from the Geological Society the Wollaston Medal. He also completed the revision of the south-eastern portion of the Greenough Geological Map of England and Wales, for the second edition, which was published in 1865. In 1868, at Norwich, he filled the Chair at the Geological Section of the British Association, dealing in a characteristic address with the geological history of the Basin of the North Sea. At the Brighton meeting in 1872 he occupied the same position, and discoursed upon the history and relations of the Wealden deposits.

In 1872, after the death of his father, Godwin-Austen went to reside at Shalford House. In spite of physical infirmity, he took an active part in the preparation of the Report of the Coal Commission, of which he was a member, and in the movement which resulted in the experimental sub-Wealden boring at Battle. He was almost to the last an energetic and useful member of the magisterial bench and of the county-boards of his native district. On the 25th November, 1884, he passed away, after a protracted illness, his death occurring in the house of his birth, the place where he had spent the earliest and latest years of his active and useful life.

Among his numerous family several sons have distinguished themselves in the military profession, and his eldest son, Lieutenant-Colonel Godwin-Austen, F.R.S., availing himself of opportunities of scientific study during many years of work upon the Topographical Survey of India, has made large contributions to our knowledge of the geology and zoology of that country. J. W. J.

John Gwyn Jeffreys was born on the 18th of January, 1809, at Swansea, where his great grandfather, his grandfather, and his father had successively practised as solicitors. His father, who had occupied a leading position in the town, died in 1815, leaving four young children, of whom the subject of this notice was the eldest. He received the chief part of his education at the Swansea Grammar School, in which he finally attained the place of "head boy," after a long competition with his rival. Having early begun to collect shells on the shore of Swansea Bay, and having been encouraged in the study of Conchology by Mr. Griffiths (Master of the Grammar School), Mr. Dillwyn, and other friends, he thenceforth made it a regular pursuit, at first as a recreation, and in later years as his chief occupation. Articled at the age of seventeen to one of the principal solicitors of his native town, he laboured diligently at his law studies, but devoted his autumnal holidays to dredging along the coast. In 1829, when only nineteen years of age, he presented to the Linnæan Society a "Synopsis of the Pulmonobranchous Mollusca of Great Britain," which was published in its Transactions; and in the following year he was elected a Fellow. In 1836 he attended the meeting of the British Association at Bristol;* and there first met Edward Forbes, with whom he afterwards formed an intimate friendship, which became valuable to both. For while Gwyn Jeffreys made many important contributions to Forbes and Hanley's classical work on the British Mollusca, his own scientific horizon was enlarged by intercourse with the most philosophic and far-seeing British naturalist of his time. Gwyn Jeffreys continued for many years to be one of the most constant attendants at the Annual Meetings of the Association; serving as Local Treasurer at its first meeting at Swansea in 1848, as President of the Biological Section at its Plymouth meeting in 1877, and as Vice-President of the Association at its second meeting at Swansea in 1880. He was elected a Fellow of the Royal Society in 1840, and served on its Council in the years 1869—1871.

Having entered into an advantageous partnership as a solicitor in Swansea, and married a daughter of R. J. Nevill, Esq., of Llangennech Park, Carmarthenshire, he applied himself assiduously to the business of his profession; but still carried on his conchological researches by systematic dredging during his vacations, at first in a row-boat, but afterwards in a yacht, which he purchased for the purpose of extending his explorations to the northern part of the British seas,

* This Bristol meeting was also the first attended by the writer of this notice, who believes that he is now the only scientific representative of the large gathering there assembled. In that gathering his old fellow-student, Edward Forbes, was always personally conspicuous, the question being continually asked, "Who is the philosopher with the long hair?"

and of working down to greater depths. His inquiries early led him to suspect that the marine Molluscan Fauna of the present time is the direct continuation of that of the later Crag period; and this idea received strong confirmation from his finding many Crag shells, supposed to have become extinct, still living in the seas around Shetland and the Hebrides.

After having published, from time to time, numerous short papers on the results of his explorations, Gwyn Jeffreys determined to bring out a systematic treatise on British Conchology, not in rivalry with the elaborate work of Forbes and Hanley, but on a less costly scale, generic types only (instead of specific) being illustrated. The first volume of this treatise appeared in 1862, and the fifth and last in 1869. He had previously removed from Swansea to London; having been called to the Bar in 1856, with the view of practising in the Court of Chancery and before Parliamentary Committees. In 1866, however, he retired altogether from legal practice, with the view of devoting his whole time to scientific work, and soon afterwards transferred his residence to Ware Priory, Herts. There, whilst taking an active interest in the business of the county (of which he became J.P., then D.L., and in 1877 High Sheriff), he continued his conchological studies; and, with his estimable and accomplished wife, gave a hospitable reception to his numerous scientific friends of all countries.

Gwyn Jeffreys's dredgings, having been hitherto prosecuted on what is now regarded as the submerged portion of the Continental platform, had not been carried deeper than 200 fathoms. But the feasibility of exploring much greater depths having been demonstrated in the experimental cruise of the "Lightning" in 1868 (when a successful haul was brought up from 650 fathoms), the "Porcupine" Expedition was next year fitted out expressly for the prosecution of still deeper explorations. As both Dr. Carpenter and Professor Wyville Thomson—who had been the joint promoters of this research—were precluded by their official duties from taking charge of the work during the earlier part of the season, they were very glad to avail themselves of Gwyn Jeffreys's offer to superintend it; and the results proved in every way satisfactory. During the first cruise the dredge was successfully worked at the then unapproached depth of 1476 fathoms, thus preparing the way for the great exploit of the expedition, the 2435 fathoms' dredging in the second cruise. Gwyn Jeffreys's Report of the First Cruise ("Proc. Roy. Soc.," Nov. 18, 1869, vol. 18, pp. 415—423) mentions, among the most remarkable novelties of his deepest dredgings, the singular Clypeastroid, now known as *Pourtalesia Jeffreysi*,* and the beautiful little *Orbitolite*

* When this was first brought up, it was supposed to be an entirely new form. On the return of the Expedition, however, Professor Wyville Thomson learned that

disk, to which (on account of its extreme tenuity) Dr. Carpenter has designated *O. tenuissima.* Both these types have recently been made the subject of special monographs; the former by Professor Lovén,* who characterises it as "the most extraordinary Echinoid hitherto known;" and the latter by Dr. Carpenter,† who finds in it the complete key to the pedigree of the Orbitoline type, and makes it the basis of a Study in the Theory of Descent. The total number of additions made by this expedition to British shells (partly of species previously known, but new to British seas,—partly of species previously known only as fossils,—and partly of species altogether new to science) was so great, that Gwyn Jeffreys spoke of them as requiring for their description an additional volume of his Conchology. This, however, he never brought out; but communicated to the Zoological Society his descriptions of new types.

In the following year (1870) Gwyn Jeffreys again undertook the charge of the earlier part of the "Porcupine" deep-sea explorations; which, it was arranged, should extend from Falmouth to the Straits of Gibraltar, along the eastern border of the Atlantic basin. Although his dredgings were not carried on at as great depths in this cruise as they had been in the previous year, yet their results were not less interesting. Thus, in one haul, at 994 fathoms off the coast of Portugal, no fewer than 186 species of shells were brought up, of which 71 were undescribed, whilst 24 were only known as fossil, less than half having been previously described as existing species. In another day's dredging in the same neighbourhood, at from 600 to 1095 fathoms' depth, several rare Siliceous Sponges and Echinoderms were found; but the great prize was a beautiful new species of *Pentacrinus* —the first of that type ever met with in temperate seas—of which a full account has recently been given by Dr. P. H. Carpenter,‡ under the specific designation *Wyville-Thomsoni,* assigned to it by its discoverer.

The prevalence of northern forms in this deep-sea Fauna confirmed Gwyn Jeffreys's previous views as to the southward extension of that Fauna into the Mediterranean; and perceiving the improbability that in the existing condition of the Strait of Gibraltar any immigration of bottom crawlers could take place, he threw out the suggestion in

a similar generic type had been obtained in the previous year in the dredgings carried on by Count Pourtales in the Gulf Stream, and had been named by Professor Alexander Agassiz *Pourtalesia miranda.* As the "Porcupine" type proved specifically different, Professor W. Thomson designated it *P. Jeffreysi.*

* "Kongl. Svenska Vetenskaps Academiens Handlingar," 1883.

† "Phil. Trans.," vol. 174, p. 551.

‡ "Report on the *Crinoidea* collected during the Voyage of H.M.S. 'Challenger,'" Part I, pp. 313–321.

his report of this cruise ("Proc. Roy. Soc.," Dec. 8, 1870, vol. 19, pp. 152—161), of a former communication between the Bay of Biscay and the Gulf of Lyons, probably in the later Tertiary epoch, nearly in the line of the Languedoc Canal.*. On giving over the charge of the Mediterranean cruise to Dr. Carpenter, Gwyn Jeffreys proceeded to Sicily *viâ* Malta, for the purpose of examining the shells found in the later Tertiaries of Sicily and Italy, preserved in the collections at Catania, Messina, Palermo, and Naples; and of comparing these with his deep-sea types.

In 1871 he visited the United States, and, through the kindness of Professor Spencer Baird, was enabled to take part in a dredging cruise off the coast of New England. When the last Arctic expedition was fitted out in 1875, and an additional ship, the "Valorous," was provided for the conveyance of stores as far as Disco Island in Baffin's Bay, Gwyn Jeffreys undertook the superintendence of the deep-sea explorations, for which provision was made on her return voyage. His report on this cruise ("Proc. Roy. Soc.," June 15, 1876, vol. 24, pp. 623–636, and vol. 25, p. 92), which includes contributions from the Rev. A. M. Norman, Dr. Macintosh, Dr. Carpenter and Professor Dickie, shows that although (in consequence of an accident to the ship) the work done was less complete than had been hoped, results of great interest, especially in regard to geographical distribution, were obtained.

In 1880, on the invitation of Professor Milne-Edwards, Gwyn Jeffreys joined the Expedition fitted out for the deep-sea exploration by the French Government, the work of which was prosecuted in the Bay of Biscay and the neighbouring portion of the Atlantic basin. After that date, though constantly occupied in the prosecution of his Conchological studies, he did not again engage in marine research.

On the death of his wife in 1881, Gwyn Jeffreys removed from Ware Priory to Kensington, where he passed the last years of his life. His health continued good, and the advance of years seemed but little to impair his usual vigour. On the evening of January 23, having just completed his 76th year, he attended a lecture given at the Royal Institution by his son-in-law, Professor Moseley, of Oxford; but on the following morning was struck down by apoplexy, and died a few hours afterwards.

While possessing an excellent general acquaintance with Marine Invertebrate Zoology, Gwyn Jeffreys's scientific position rested on the thoroughness of his knowledge of Conchology, in which department he came to hold a highly distinguished rank. He had a keen

* In considering this suggestion, however, it should be borne in mind that all marine Mollusca have free-swimming larvæ; and that, as these live near the surface, they would be liable to be carried into the Mediterranean by the Gibraltar current.

eye for minute distinctions, a methodical habit of mind, scrupulous exactness, and an excellent memory. He spared no pains to clear up a doubtful point, and never satisfied himself with imperfect knowledge, where there was more to be acquired. Continuing to believe in the permanence of species, and opposing the doctrine of evolution, he nevertheless fully recognised the frequency of a wide range of variation; and his collection, instead of being restricted to type-forms, contained many interesting series of varietal modifications. How important is the careful and systematic study of Shells, in relation to the existing geographical distribution of Molluscan species (both terrestrial and marine), and, through this, to the elucidation of the past history of the globe, would not need to be here pointed out, if it were not that among the present generation of naturalists such study finds comparatively little appreciation. It should never be forgotten that (to go no further back in the history of Geology) it was entirely upon the Conchological comparisons of Deshayes, that Lyell founded his division of Tertiary formations into Eocene, Miocene, and Pliocene —a division which has stood the test of fifty years' thorough scrutiny. And it will not be for the advantage of Science, if Conchology should ever cease to attract competent workers. No better model could be set forth of what Conchological work should be, than that which is presented in Gwyn Jeffreys's life-long labours, whose results are contained in the fifty-five years' series of papers (considerably exceeding 100 in number) which he communicated to the Societies of which he was a Fellow, and to the pages of scientific Journals.

It should not be left unmentioned that Gwyn Jeffreys's excellent business-habits caused his financial services to be sought by the Linnæan and Geological Societies, the treasurership of both of which he held for several years; and that as treasurer also of the Royal Society Club, his social qualities did much to promote its prosperity.

W. B. C.

John Christopher Augustus Voelcker was born on September 24th, 1822, at Frankfort-on-the-Maine. He was the fifth son, in a family of seven sons and one daughter, of Frederick Adolphus Voelcker, a merchant of that city, who died when his fifth son was only eleven years old. During his years of boyhood, Augustus suffered from very delicate health, which greatly retarded his early education. This he obtained at a private school in the town. About the age of twenty-two he went to the University of Göttingen, chiefly for the purpose of studying chemistry under, and of working in the laboratory of, Professor Wöhler.

At Göttingen he took the degree of Doctor of Philosophy, in 1846, the subject of his inaugural dissertation being the composition of tortoiseshell.

He also appears to have devoted his attention to the investigation of some of the compounds of manganese, and of some other metals, on which, in 1846, he published papers. In the same year he also published a paper on the occurrence of mannite in the roots of *Triticum repens;* and one on the analysis of poppy-oil.

It was also whilst he was still at Göttingen, that Professor Mulder, the distinguished Dutch chemist, paid a visit to Wöhler at that place; and, on Wöhler's recommendation, Mulder engaged Voelcker as his principal assistant, who accordingly returned with him to Utrecht, where he remained for some time. Dr. Voelcker assisted Mulder in his various investigations, and it was doubtless this work, and the connexions into which it led him, that gave a direction to his future studies and labours.

At Utrecht, Dr. Voelcker commenced the investigation of some of the albuminous compounds, and he continued the inquiry from time to time for some years afterwards; but the only record of this work which has come under my notice is in papers read at the meetings of the British Association for the Advancement of Science: in 1855—"On Caseine, and a Method of Determining Sulphur and Phosphorus in Organic Compounds in one Operation;" and in 1857—"On the Proportion of Organic Phosphorus in Legumine."

During Dr. Voelcker's stay at Utrecht, Professor James F. W. Johnston, of Edinburgh, who was Chemist to the Agricultural Chemistry Association of Scotland, afterwards incorporated with the Highland and Agricultural Society of Scotland, paid a visit to Mulder, and he induced Dr. Voelcker to go to Edinburgh to take charge of the laboratory of that Association. He went to Edinburgh in February, 1847, and remained there until August, 1849; excepting that from November, 1848, to February, 1849, he spent at Durham, at the University of which place Johnston was Professor of Chemistry, and for whom he lectured and worked in the laboratory there.

At Edinburgh the whole responsibility of the position, both as analyst and consulting chemist, frequently devolved upon him, Professor Johnston spending much of his time at Durham, or being otherwise engaged. It was under these circumstances that he first gained experience in the requirements of practical agriculture; for it was here that for the first time he found himself constantly in communication with practical farmers, learning from them their wants, and investigating and advising on the problems they brought before him for his solution. Trained in analysis in the best schools of the time, himself an acute observer, and having an eminently practical turn of mind, the responsibility of his position greatly tended to develop his powers, and to give him that self-reliance which was his characteristic through life, and which, thoroughly sustained by knowledge, industry, and conscientiousness, contributed in no small degree

to his success as a teacher, a scientific adviser to the practical farmer, and in his profession as a Consulting Chemist generally.

In August, 1849, after spending about two and a half years at Edinburgh, Dr. Voelcker was appointed Professor of Chemistry at the Royal Agricultural College, Cirencester; and from this time began a still more active life of lecturing, writing, and experimenting. His income at the College was small, but he supplemented it by writing, and by analytical work. The articles on chemical subjects in "Morton's Cyclopædia of Agriculture," from the letter M to the end, were contributed by him.

It was about, or soon after this time, that he contributed papers on various subjects of investigation to some Scotch scientific and agricultural journals. For example, to the "Edinburgh New Philosophical Journal," "Analysis of the Anthracite of the Calton Hill, Edinburgh;" to the "Annals and Magazine of Natural History," "On the Chemical Composition of the Fluid in the Ascidia of Nepenthes;" "On the Composition of the Ash of *Armeria maritima* growing in Different Localities, with Remarks on the Geological Distribution of that Plant, and on the Presence of Fluorine in Plants;" "On the Watery Secretion of the Leaves and Stems of the Ice Plant." To the "Transactions of the Highland and Agricultural Society of Scotland," "The Chemical Composition of the Seed of *Chenopodium quinoa;*" "Composition of House Coal Soot;" "On Artificial Manures in General and Bone Manure in Particular;" "The Effects of Burnt Clay as a Manure;" "On the Comparative Value of White Scottish and Black English Oats;" and "On the Composition of Rice Meal."

In 1852 Dr. Voelcker went to Frankfort to be married, and he returned to Cirencester with his wife. Mrs. Voelcker, four sons and one daughter, survive to mourn his loss.

In 1855 Professor Voelcker was appointed Consulting Chemist to the Bath and West of England Agricultural Society, and he held the office up to the time of his death, a period of nearly thirty years. In this capacity he gave lectures at various places from time to time, instituted field and other experiments, conducted much laboratory investigation, and contributed papers to the Journal of the Society.

In 1852 his first paper in the "Journal of the Royal Agricultural Society of England" appeared. He also contributed one in 1855, one in 1856, and one in 1857. In that year, 1857, he was appointed Consulting Chemist to the Society; and from that time to the date of his death, about twenty-seven years, he contributed one or more papers to every half-yearly number of the Society's Journal.

For about six years after his appointment as Consulting Chemist to the Society, he still retained his Professorship at Cirencester; and he availed himself of the opportunities which the College farm

afforded for carrying out various experiments at the homestead and in the field, which were supplemented by collateral laboratory investigations. He had already commenced an investigation into the composition of farmyard manure, and as to the changes it undergoes under various circumstances, such as in contact with different soils.

From the results obtained in these inquiries, he was led to investigate the absorptive powers of different soils of known composition; and he showed that the most important soluble constituents of manures are rendered less soluble, but not quite insoluble, when applied to the soil.

In the course of his inquiries, he instituted numerous field experiments with different manures, on different crops, on the College farm; and he also endeavoured to enlist the co-operation of intelligent practical farmers in different localities in the conduct of field experiments.

In 1878, he became responsible for the conduct of the systematic series of field experiments which, in the previous year, had been commenced at Woburn, on behalf of the Royal Agricultural Society, at the instance, and at the cost, of His Grace the Duke of Bedford. To the last he took the deepest interest in the management and in the results of these experiments.

Very soon after he had devoted himself to agricultural chemistry, Dr. Voelcker commenced to pay attention to the various aspects of the subject of the feeding of animals. He had not the same facilities, either for conducting feeding experiments himself, or for arranging with others to conduct them, that he had in case of field experiments with manures. He, however, not only wrote and lectured on the chemistry of the feeding process, but he analysed a very large number of food stuffs, both home-grown and imported. He determined the composition, in much detail, of most of the crops grown on the farm as food, of new plants proposed to be grown as food-crops, of hay, of various descriptions of straw, of certain refuse matters, and so on; discussing at length their actual and comparative feeding value, as deduced from the results of his laboratory investigations.

But perhaps the most essential service he rendered, in connexion with the composition and value of food-stuffs, was by his most elaborate investigations, microscopic and chemical, of the various matters entering into the composition of feeding-cakes, by his numerous analyses of the various cakes themselves, and by his fearless and persistent exposure of what he considered injurious, or against the feeder's interest, in the manufacture or composition of such articles, whether resulting from carelessness, ignorance, or fraud.

The subject of Milk and the Dairy, which is one of rapidly growing importance to the British farmer, was early taken up by Dr. Voelcker,

his first paper on the subject appearing in 1861. In connexion with this subject he has executed a great amount of analytical work, made many experiments in dairy management, given several lectures, and published not a few papers recording existing knowledge, and the numerous results of his own investigations.

In all his work and publications, the thorough manner in which he sought to elucidate the connexion between practice and science is conspicuous. Many other questions of agricultural interest than those which have been referred to, engaged his attention, such as—the properties of soils in other aspects than those which have been mentioned; the composition and value of town sewage, and also of earth-closet manure; the chemistry of sugar-beet; and the chemistry of drinking-waters.

From time to time Dr. Voelcker contributed papers to the Chemical Society, and in some cases he gave the same results in less technical form in the "Journal of the Royal Agricultural Society." Of those communicated to both journals, the one involving by far the largest amount of laboratory investigation, and leading to the most important conclusions, both practical and scientific, related to the composition of the waters of land-drainage, and to the loss of plant-food thereby.

He was elected a member of the Chemical Society in 1849; was several times a member of the Council of the Society; and was one of its Vice-Presidents at the time of his death. He was elected a Fellow of the Royal Society in 1870. He was one of the founders, and one of the first Vice-Presidents, of the Institute of Chemistry of Great Britain and Ireland, established in 1877.

On coming to London in 1863, in addition to his duties as Consulting Chemist to the Royal Agricultural Society, Dr. Voelcker commenced private practice as a Consulting Chemist, and very soon acquired considerable repute in this capacity, and gained a very extensive practice, not only in connexion with agriculture, but with many other industries, being frequently called upon to give evidence before Parliamentary Committees or Royal Commissions.

There can be little doubt that his life was shortened by overwork. On Sunday, December 23, 1883, he had an attack of paralysis, not severe, but such as to indicate that absolute rest was essential for some considerable time; the Council of the Royal Agricultural Society accordingly requested him to devote six months to the restoration of his health. As soon, however, as he felt some recovery of strength, it was impossible to restrain him from returning more or less to his active duties. He had no return of paralysis, but in August, 1884, symptoms of heart disease, with other complications, supervened. From this time he never really rallied, and he died on the morning of December 5. For some weeks he had suffered intensely; and not

many days before his death, his sufferings were indeed very painful to witness. His mind was, however, perfectly clear; he fully recognised his position, and was entirely resigned to it. He passed away quietly, and without any pain towards the last.—J. H. G.

CHARLES ADOLPHE WURTZ was born at Wolfisheim, a village near Strasburg, on the 26th November, 1817.

His father, Jean Jacques Wurtz, the Protestant minister at Wolfisheim, was a man of considerable literary culture, but of a somewhat gloomy disposition. His mother, Sophie Kreiss, was singularly cheerful and sweet-tempered, had a sound clear head, and was most conscientious in the discharge of her duties in life. She was the intimate friend and confidant of her son until her death, which took place quite recently, namely in 1878.

Young Adolphe spent his early childhood at Wolfisheim, and he probably owed to his country life as a child the robust health which he enjoyed in after life.

As Wolfisheim was not many miles from Strasburg, the Wurtz family, although living in the country, were by no means isolated, and had frequent opportunities of enjoying the society of their various friends and relations. Besides other friends, Madame Wurtz's two brothers, Théodore and Adolphe Kreiss, would often come out to spend Sunday at the parsonage, and the intelligent conversation of these two distinguished men probably contributed in no small measure to the moral and intellectual development of young Wurtz.

In the year 1826 Jean Jacques Wurtz was appointed to the church of St. Pierre-le-Jeune, in Strasburg, and Adolphe now joined the classes of the Gymnase Protestant, a school of secondary instruction, founded by Jean Sturm at the period of the Reformation.

As a schoolboy Adolphe Wurtz did not specially distinguish himself. During the eight years he attended the Gymnasium he obtained several prizes for diligence, one for geography, one for memory and elocution, besides mentions for history and geography, for Latin and Greek translation, mathematics, and French verse. He appears to have worked industriously and steadily at the various subjects taught him, but not to have particularly distinguished himself in any one. Hence it is not surprising that his father, with his morose disposition, should on more than one occasion have told the boy that he would never do anything remarkable in life.

A free course of botany was open to the pupils of the Gymnase Protestant, and in 1828 young Wurtz attended this course, which doubtless contributed to the development of his faculties of observation, and to give him his taste for natural history. Years afterwards, when fully entered upon his career as a chemist, he still took a pleasure in reading the works of the naturalist Oken.

Although, owing to his father's poor circumstances, and more especially to his gloomy disposition, Adolphe Wurtz's home was by no means a cheerful one, the young people spent many pleasant hours at the house of their grandfather, M. Kreiss. Besides this kind and excellent man, there were the two uncles, Théodore and Adolphe, the former of whom became to them as a second father, on the death of his brother-in-law. M. Théodore Kreiss always took the warmest interest in their studies, and was amply rewarded in after years by their gratitude and affection, and by their success in life.

The young people usually passed their holidays at the house of a great aunt at Rothau, in the Ban de la Roche. The excursions made on these occasions amongst the mountains and woods of the neighbourhood, and also to various factories, and to the mines and iron-works of Armont, were amongst Wurtz's pleasantest recollections in after life.

Adolphe Wurtz left the Gymnase in 1834, after taking the degree of Bachélier ès Lettres. It was his father's earnest wish that he should now attend a course of instruction as a preparation for theology, but the youth had already contracted a love of science. For some time past he had been making chemical and physical experiments in the laundry attached to his father's house. His mother made no objection to this, but his father looked upon such pursuits as both a waste of time and of money, and occasionally even went so far as actually to demolish the little brick furnaces which the future chemist had the ingenuity to build up.

Wurtz's strong inclination for chemistry was no doubt fostered by his intercourse with Emil Kopp—afterwards the distinguished chemist, who in later years was one of his fellow workers at the Dictionnaire de Chimie.

The youth at last announced that he wished to make chemistry his profession, but his father refused his permission, insisting on his son taking up either theology or medicine. The youth decided on the latter, and this selection of medicine was probably influenced by the opportunity it would afford him of pursuing his favourite study, as he would have to attend a course of chemistry, and would have access to a laboratory.

Wurtz soon became (in 1835) second and before long full assistant in the chemical and pharmaceutical department. At a competitive examination in 1839, he was appointed Chef des Travaux Chimiques de la Faculté, on which occasion he wrote an essay: "Histoire Chimique de la bile à l'état sain et à l'état pathologique." He fulfilled the duties of this new post under the direction of M. Caillot until he left Strasburg.

It was thus that whilst regularly and quietly pursuing his medical studies and passing the necessary examinations, Wurtz became a

chemist. All this earnest and arduous work did not, however, prevent him from joining in the various amusements going on in his family. He had a good voice and was fond of singing, and in such a musical city as Strasburg there was plenty of scope for this accomplishment. In the year 1843 he took the degree of Doctor of Medicine, and on this occasion he read a thesis "Essai sur l'albumine et la fibrine," which gained him a medal from the Faculté. He now obtained the permission of his family to go for a year to Giessen, where Liebig had opened the first laboratory for students. At Giessen he became acquainted with Dr. A. W. Hofmann, with whom he maintained a friendly intercourse through life. Here also he became intimate with Strecker, with Hermann Kopp, and others. Liebig received him in the most friendly manner, and entrusted him with the translation of some of his papers into French. These translations, which were sent to Paris for insertion in the "Annales de Chemie et de Physique," were the means of bringing Wurtz into contact with some of the leading French men of science, and in particular with Dumas.

It was in Liebig's laboratory that Wurtz began his investigation of hypo-phosphorous acid.

On leaving Giessen he made a rapid trip to Vienna before returning to Strasburg. In 1844 he left Strasburg for good, and went to Paris with letters of introduction from Liebig. There he worked for a short time in Balard's laboratory, but very soon removed to the laboratory of Dumas, in the Rue Cuvier, where were also working Messrs. Cahours, Mellens, Lewy, Leblanc and Bouis.

In 1845 he became assistant to Dumas at the École de Médecine, and it was whilst assisting Dumas that he made his great discovery of the compound ammonias, which had such an important influence on the progress of chemistry, and which gave the clue to the constitution of the vegetable alkaloids. It was also in Dumas' laboratory that he completed the beautiful investigation which he had begun at Giessen of the constitution of the hypophosphites.

About this time he gave some instruction to M. Eugène Caventou, who became one of his most intimate friends. The father of this young man, M. Caventou, the discoverer of quinine, was not slow to perceive the merits of young Wurtz, used frequently to invite him to his house, and did his best to support him by his influence on various occasions.

From 1845 to 1850, Wurtz held the appointment of Chef de Travaux Chimiques of the 3rd and 4th year students at the École Centrale des Arts et Manufactures.

In the year 1847, Wurtz distinguished himself greatly at the competition for the Agrégation de Chimie. One *leçon* which he gave on this occasion, "Sur les Corps Pyrogénés," earned high praise.

In 1845 he was employed in his capacity of Agrégé to give Dumas' course of lectures on Organic Chemistry, Dumas being prevented by his political and administrative occupations from attending to his professional duties.

Wurtz was at this time working in a dark and inconvenient laboratory, situated in the Practical School of the Faculté de Médecine in the Musée Dupuytren. On taking possession of it he found it in such a state that the first thing he did was to go with his assistant, M. A. Rigout, and buy a pot of colour and some brushes, with which he himself painted the black and dirty walls. He was not only particular about neatness and exactness in his work, but attached importance to having a light, cheerful, well-kept laboratory.

Desirous of getting more suitable conditions for work, Wurtz joined in 1850 with two young chemists, Messrs. Charles Dolfus and Verdeil, just returned from a course of practical chemistry under Liebig—to open a laboratory in the Rue Garancière. The three friends were each to carry on their separate experiments and to take a few pupils, Charles Dolfus contributing the money needed for the undertaking. Wurtz was the real scientific director of the enterprise, and this was practically the beginning of the laboratory in which he accomplished such great work, and in which so many chemists have been trained. Amongst those who worked under his direction in the Rue Garancière, we may mention Mr. Marcet, well-known for his labours in the department of Biological Science, M. E. Risler, who was already working at the applications of Agricultural Chemistry, and who afterwards became Director of the Institut National Agronomique, and Adolphe Perrot, afterwards Wurtz's assistant at the Faculté de Médecine.

But though this enterprise of the three young chemists was undertaken to supply a real want—it was not successful; unfortunately the young men were not long able to keep possession of their laboratory, for the house was very soon sold to a printer; and they were obliged to quit, and to sell off the fittings, &c., which had been a considerable expense to them.

It was at about this period that Wurtz became more or less intimately acquainted with various men afterwards distinguished in science or literature. Most of these were members of the Société Philomathque, sometimes called the Ante-Chamber of the Institute. Some of these friends were in the habit of meeting after dinner at the Café Procope before going to the meetings of the Society in the Rue Anjou-Dauphine. Occasionally it would happen that the conversation was so interesting that they prolonged their sitting at the café, and did not go at all to the meeting of the Society; but this was no loss to science when the party consisted of such men as

Wurtz, Foucault, Verdet and Brégnet, Himly, Regnault, Robin and Serret.

When the Institut Agronomique was founded at Versailles in 1850, Wurtz was appointed Professor of Chemistry in it. His appointment, however, was not of long duration, for the new institution was suppressed in 1852 by the Prince President. He lost his appointment just as he was about to marry, and it was 25 years before this institution, so much needed for the promotion of agricultural science, was re-established.

Wurtz was ere long amply compensated for this disappointment by his election as Professor at the Faculté de Médecine in 1853. Dumas had resigned his chair, and Orfila, who had been Professor of Mineral and Toxicological Chemistry, was dead. The two chairs were now united and Wurtz appointed to the post. It might seem a difficult task to replace two men of such talent and reputation, but Wurtz was equal to the emergency, and for 30 years his lecture room was crowded by students who flocked to hear him, attracted by his lucidity and masterly exposition. Whilst lecturing, he would go from the table where his experiments were made, to the black board, all the time speaking with eloquence and vivacity, talking of chemical combinations with as much enthusiasm as though his subject had been the welfare of a State. He always prepared his lectures carefully beforehand, and more and more carefully as years went on.

The principal laboratory where Wurtz worked surrounded by his students had been taken in from the little lecture room of the Faculté. It was lofty, with a vaulted roof, and very light, and held about a dozen students besides the Professor. The balances, which were placed on a little stand in the amphitheatre itself, were not accessible while the lectures were going on. Several adjoining rooms, which were at first used for special experiments on a large scale, such as combustions, &c., had afterwards to be given up for the ordinary work for the additional students who came to Wurtz's laboratory.

Wurtz had only a very moderate sum allowed him for the expense of his course, yet he managed with this and the help of some subscriptions from his pupils not only to buy apparatus and substances, but to defray the expenses of various alterations and improvements. His attempts to get a larger allowance for these purposes never succeeded until years afterwards, when as Dean he obtained a rather larger salary, and first one, then a second assistant.

In the year 1862, being then in London, on the occasion of the Universal Exhibition, Wurtz gave an address to the Chemical Society, "Sur l'Oxyde d'Éthylène considéré comme un lien entre la Chimie Organique et la Chimie Minérale."

In 1864, he being then in his 47th year, he was elected a Foreign Member of the Royal Society.

In 1866 he accepted the post of Dean at the Faculté de Médecine. In thus consenting to sacrifice a part of his valuable time to administrative occupations, it was in the hope of promoting the development of scientific instruction in the Faculté. He succeeded, in fact, in reorganising it, got all the practical work placed on a new footing, especially that of chemistry, obtained a laboratory of Biological Chemistry for his pupil, M. Gautier, and that laboratories should be put at the disposal of the clinical professors in the hospitals. He was an active advocate for the admission of women on an equal footing with men to the classes and examinations. He likewise took a considerable part in the planning and execution of the new buildings of the Faculté, and of the École Pratique. His high-minded courage enabled him to pull happily through a period of trouble, and to retain the office of Dean until a period of tranquillity ensued.

On two different occasions, in 1868 and in 1878, Wurtz visited the principal German and Austrian University centres, bringing back with him numerous documents, by the help of which he drew up two elaborate reports on Foreign Chemical, Physiological, Anatomical, and Pathological Anatomical Laboratories.

Wurtz had held his professorship at the École de Médecine for twenty-five years, when in 1874 he was appointed to the new Chair of Organic Chemistry, at the Sorbonne. He then resigned the office of Dean at the École de Médecine. He was named Honorary Dean, a distinction well earned by his many and long services, especially by the courage he had shown during the disastrous time of the Commune, never quitting his post until summoned to Versailles.

Wurtz had long wished for the opportunity of teaching the higher theories of chemistry, which he of course could not do to a class consisting chiefly of medical students, who for the most part took no interest in the subject, except in as far as it was necessary for passing their examination.

At the Sorbonne, Wurtz had no laboratory, and his experiments had to be prepared in his old laboratory at the École de Médecine, and all the substances and apparatus to be carried to and fro for each lecture. Thanks to the energy of the Professor, and to the efficiency of his able assistants, Messrs. Salet and Œchsner de Coninck, the course did not suffer from this unusual arrangement. It was only in the last months of 1881, after the death of Henri Sainte-Claire Deville, that a small laboratory was given to Wurtz for preparing his lecture experiments. A new and more suitable laboratory was being built for him in the Avenue de l'Observatoire, according to plans drawn up by himself, pending the completion of which Wurtz and his students remained at the École de Médecine, and he was looking forward during the last few months of his life to occupying the new buildings at the commencement of the following session.

During the siege of Paris Wurtz took an active interest in the fate of the inhabitants of Alsace-Lorraine, who had crowded to Paris. He was one of those who took part in the establishment of the Société de Protection des Alsaciens-Lorrains, which has been the means of relieving so much suffering, and of founding in Algiers three prosperous villages, peopled by refugees from those two provinces. He was also one of the first shareholders in the École Alsacienne, in which school the principles of instruction of the Gymnase Protestant of Strasburg have been adopted with much success. He was an active member of the committees of several charitable and other societies. He frequently spoke at the public meetings of the Société Protestant de Prévoyance et de Secours Mutuels, of which he was Vice-President, M. Léon Say being President.

In the year 1880 he went to Bordeaux to take part as a member of the Committee at the annual meeting of the Colonial Agricole de Sainte-Foy. At this meeting he delivered a most eloquent address, giving an account of the life of Felix Vernes, and of his services to his country during the siege and to French Protestantism.

Wurtz himself had worked most actively during the siege, both in the ambulances and on the field of battle. After the battle of Buzenval, the Société Française de Secours aux Blessés, of which he was on the Council, entrusted him with the painful task of finding the body of Henry Regnault. On the 23rd of January he reported to the Académie des Sciences his failure to find the remains of the son of his illustrious colleague. As we now know, it was in the cemetery of Père la Chaise that the body was at last recognised, amongst a crowd of others, on the 24th January.

Wurtz remained to the end of his life firmly attached to the religious belief in which he had been brought up. He was assiduous in his attendance at the meetings of the Consistory and the Synods. He contributed greatly to the reorganisation in Paris of the Strasburg Faculté de Théologie Protestante, in which he continued to take an active interest, and he also accepted the presidency of a society which was founded for the encouragement of theological study.

He was of course a member of all the principal learned and scientific societies, both in France and abroad. He became Vice-President of the Académie des Sciences in 1880, and presided at the sittings during the following year. His numerous brilliant and scientific labours were appreciated abroad at least as fully as in France, and certainly sooner. He became a Foreign Member of the Royal Society before he was admitted to the Institute. True it is that between the election of Balard to the Académie des Sciences, in 1844, and that of Wurtz in place of Pelouze, in 1867, there was only one other election, that of 1857, when Frémy was chosen in pre-

ference to Henri Sainte-Claire Deville, Wurtz, Berthelot, and Cahours The Académie had already, however, given Wurtz every other distinction it had to bestow. In 1859 it had awarded to him and his friend Cahours jointly the Jecker Prize; in 1864 it had again conferred on him the Jecker Prize; and in 1865 the great Biennial Prize of 20,000 francs was voted to him by the Institute.

In 1881, the Copley Medal was awarded to him by the Royal Society.

During the siege of Paris he punctually attended the daily meetings of the Comité Supérieure de Hygiène. At these meetings had to be considered the question of supplying food to the city, of the best means of resisting epidemics, and of diminishing as far as possible the immense mortality caused by the investment. Among the members of this committee were H. Sainte-Claire Deville, Gubler, Behier, &c. Wurtz became President in 1879.

He was also an active member of the Commission des Hôpitaux Civils et Militaires.

He had been a member of the Académie de Médecine since 1856. In 1871 he was chosen President, and during the whole of that gloomy year he regularly took the chair at the meetings, with one exception, that of the 23rd of May, during the disastrous period of the Commune. He took an important part in the discussions which were held in 1871, on vinage; in 1874, on the water of Paris; on the phenomenon of fermentation in cells; on the products of cinchonine, &c.

In 1877, during the rebuilding of the Faculté de Médecine, Wurtz moved into a provisional laboratory, which was arranged for him in the old houses facing the Rue des Écoles and the Rue Hautefeuille. This new laboratory was more commodious and better arranged than the old one, and as there were more rooms, a better distribution of the work could be made. Wurtz now had his own private laboratory, and he had also the gratification of being able to give a place in the laboratory to his former teacher, Professor Caillot, who had left Strasburg when the Germans took possession of Alsace.

It was here that Wurtz worked during the remaining years of his life, amidst a larger number of students than ever, and a little group of disciples who had gathered around him.

About the year 1856 there existed in Paris a society of young chemists, who used to meet for mutual instruction. In 1858 Wurtz conceived the happy idea of transforming this association into a learned society. He succeeded in obtaining the concurrence of Dumas, Balard, Sainte-Claire Deville, Berthelot, Thénard, Pasteur, Cahours, and others in this object. He organised the "Bulletin," a periodical in which are published the papers read before the Society, and also the "Répertoire de Chimie Pure," which gives *résumés* of chemical

work published both in France and abroad.* To this was added a "Répertoire de Chimie Appliquée," under the direction of M. Barreswil. From the time of its foundation the Société Chimique de Paris took a high rank amongst scientific societies. Wurtz himself gave several lectures at the Society: one in 1860, "L'Histoire Générale des Glycols;" three in 1863, "Sur quelques points de Philosophie Chimiques;" and one in 1883, on "Aldol," with his usual vivacity and enthusiasm. He had been chosen Secretary of the Society at the time of its foundation, and he several times filled the office of President. He was a frequent attendant at the meetings of the Society, and often gave there his most interesting papers.

Wurtz also organised in his laboratory a set of conferences, which usually took place on Saturday afternoons, where sometimes he or his students, or occasionally distinguished chemists of France and other countries, gave an account of recent researches, and showed the more important experiments.

In 1872 he took an active part in the formation of an Association for the Advancement of Science, similar to our British Association, which has done so much for Great Britain. This new French Association held its first Congress at Bordeaux, in 1872. Wurtz continued to watch assiduously over its welfare. In 1874 he presided at the meeting at Lille, and on this occasion he gave the Association an important and interesting discourse on "La Théorie des Atomes dans la Conception Générale du Monde."

In 1875 Wurtz had been persuaded to accept the office of Maire of the VII^{e} Arrondissement of Paris. He fulfilled the duties of this post with his usual zeal and devotion, until 1881, when he was proposed for election to the Senate by the Centre Gauche, and elected without opposition. On the occasion of his nomination to the Senate his numerous pupils, both French and foreign, took occasion to offer him a testimonial of gratitude and affection. They presented him with a copy in bronze of Barrias' statue of Bernard Pallissy, on the pedestal of which was engraved a dedication, with the names of 111 present and former pupils, amongst which we note Ch. Friedel, Lecoq de Boisbaudran, J. M. Crafts, Ladenburg, &c., &c.

During the short time that he was a member of the Senate, Wurtz appears to have spoken only once, namely, at the discussion of the law for permitting the importation into France of the American salted meats. He drew up for the Senate on this occasion an exhaustive report on the Trichini, collecting together for the first time all the experience of France on the subject.

In 1878 Wurtz gave the Faraday Lecture to the London Chemical Society. He selected for his subject "La Constitution de la Matière à l'état Gazeux," dwelling especially on the beautiful researches of Faraday on the liquefaction of gases. He was warmly welcomed by

his scientific friends in London, and returned to Paris well pleased with his reception.

Wurtz had married in 1852 a lady of some fortune, with whom he had been on terms of friendship since childhood. He had four children. His daughters are both married, Marie, the eldest, being the wife of her father's friend and assistant, M. Œchsner de Coninck, and he had during his last years the happiness of seeing grand-children around him.

Of his two sons, the elder, Robert, is studying medicine, and the second has passed through the École Polytechnique, with a view to a military career.

Wurtz's family circle did not however stop here. On the death of her sister, Madame Oppermann, Madame Adolphe Wurtz undertook the superintendence of the education of her four nieces, and when these young ladies afterwards lost their father, the three who were unmarried became inmates of Wurtz's house until their marriage.

Wurtz's mother, Madame Jean Jacques Wurtz, had remained for many years in Strasburg with her brother, Professor Théodore Kreiss, but, after her brother's death, she came to reside in Paris with her distinguished son. Amiable and cheerful in spite of her deafness, the only infirmity of her old age, she was thoroughly happy in the midst of the charming family circle, of which her son was the chief ornament. Wurtz's brother, Théodore, had also come, with his wife and children, to live in Paris, and with the exception of his sister, Madame Grüner, all the family were now together.

Wurtz took great delight in receiving his friends at his home, and besides his numerous friends and colleagues, his pupils often visited him either in Paris or at the country places the family inhabited during the summer months. A few years ago, he bought a charming place called Fromenteau, near Juvisy in the Seine Valley, where he thoroughly enjoyed receiving his friends, and sharing with them the pleasures of a country life.

Wurtz had, with few exceptions, enjoyed excellent health, but in 1867, owing to the fatigue of the work he had undertaken in connexion with the exhibition, he had fallen ill. The repose of the vacation however soon restored him to his usual health. He had always kept up his habit of taking plenty of exercise, whether by long walks, fishing, swimming, shooting, or gymnastics. Like most chemists, he had not escaped laboratory accidents. On one occasion a violent explosion caused an injury to one eye, which resulted in the formation of a cataract. After some years, however, his sight was restored by an operation.

Towards the end of 1883-84, his friends remarked that he showed signs of fatigue. Still he was as active as ever. In March, 1884, before resuming his course of lectures at the Sorbonne, he went for a

few days to stay with one of his daughters at Cannes. There he had the pleasure of seeing once more his friend Dumas, who was apparently in good health. Very shortly after his return, he heard of Dumas' death. He had just then business at Liège. He hurried through this as early as possible, for the sake of attending the funeral of his old master, and was imprudent enough to pass two consecutive nights in railway trains. On reaching home, he got a notice that he was expected to give an address over the grave of Dumas, on behalf of the Faculté des Sciences et Médecine. He set to work at once to compose the eloquent discourse which he delivered on this occasion. But the strain on his powers was too great, and his friends noticed with pain his altered appearance. Still he went on with his work, and resumed his lectures, though with visible effort. He gave his last lecture on the 27th April with his usual animation, but at the end was in a fainting state. He was now obliged to give up, and, as he himself remarked with some pride, it was the first time during a professorship of thirty years that he had put up a notice announcing that his state of health prevented his lecturing. Still his condition was not considered serious, and he himself did not appear to have any misgivings about his health. He was full of the idea of replacing his illustrious master, Dumas, as Secrétaire Perpetuel of the Académie des Sciences. He consulted some of his friends on this subject. They seeing that he was overdone by his work, yet feeling how desirable it was that he should hold this appointment, urged him to apply; but at the same time to give up more than an equivalent part of his other work. This he promised to do, and they still hoped to see him at the head of the Académie. But disease was making rapid progress, and he expired on the 12th of May, after a few days' illness, and only one month after paying his last tribute to the memory of Dumas.

He was followed from his home in the Boulevard St. Germain to his grave in Père-la-Chaise by an immense procession, consisting of official deputations from the Sénat, the Institut, and various learned institutions with which he was connected, and of hundreds of students principally from the École de Médecine and the Faculté des Sciences.

The labours of Wurtz have exerted a powerful influence on the development of chemistry. Among his most important discoveries may be mentioned those of the compound ammonias and the glycols, by which two extensive and fertile provinces were opened up in organic chemistry. His researches on the mixed alcohol radicals threw great light on the constitution of hydrocarbons, which had been the subject of a long controversy. He also made important experiments on dissociation. He discovered butylic alcohol. Other important investigations were his synthesis of glycerine, and of neurine, his elaborate investigation of lactic acid, his researches on the poly-

basic acids, and on the production of aldol and its beautiful derivatives.

The classical investigations with which Wurtz enriched science form but a small part of his work. He was one of the few French chemists who founded a school, and the greater number of the present generation of French chemists were his students.

It might well be supposed that the labours of this great investigator and teacher who had, in addition to his professional work, much official business and a considerable social position, would have left him neither energy nor leisure for other work. But he found time for much else. Besides being a brilliant lecturer and laboratory teacher, he excelled as a writer. The ease and rapidity with which he wrote induced him to undertake various literary works. He brought out his "Dictionnaire de Chimie" in a marvellously short time, with the aid of friends and pupils. The elegantly written little book "La Théorie Atomique" has long since been translated into all the chief modern languages. The "Leçons Élémentaires de Chimie Moderne" is a model of lucid exposition.

The fundamental feature of Wurtz's character was a love of truth. With this love of truth was combined a modest estimate of his own merits, accompanied by the most heartfelt and generous appreciation of the labours of others.

A. W. W.

William Spottiswoode was born in London on January 11th, 1825. He was descended from an old and distinguished Scottish family, of whom, perhaps, the most notable character was John Spotiswood, the Archbishop of St. Andrew's, who crowned Charles I at Holyrood, and was the author of "A History of the Church of Scotland." Andrew Spottiswoode, the father of William, was himself a man of no ordinary attainments; he represented Colchester for some time in Parliament, and in 1831 was admitted into partnership with George Eyre as one of Her Majesty's printers. His wife, William's mother, was of the Longman family, well known from its connexion with the celebrated publishing firm.

Of the early days of William Spottiswoode there is but little to tell. His school life began at Laleham under a brother of Dean Buckland. From Laleham he went to Eton, but his stay there was short, as the first recorded development of his scientific tastes resulted in an explosion which, though effecting no damage to his moral reputation, was deemed inconsistent with sound discipline. He was accordingly moved to Harrow, then under Dr. Wordsworth, and was there placed in the upper "shell." After continuing three years at Harrow, where he had the reputation for being studious and thoughtful, he in 1842 obtained a Lyon Scholarship and went to Balliol College, Oxford.

His mathematical tutors there were Dr. Temple, the present Bishop of London, and afterwards the Rev. Bartholomew Price, who had the highest opinion of his industry and power of work. His range of reading is said to have been very extensive. In 1845 he took his B.A. degree as a first-class in mathematics, and in 1846–7 gained successively the Senior University and the Johnson's Mathematical Scholarships.

In 1846 Mr. Spottiswoode left Oxford to take his father's place as Queen's printer, but he kept up his connexion with the University, delivering a course of lectures at his college on solid geometry, and acting in 1857–8 as Examiner in the Mathematical Schools.

The business of the Queen's Printing Office continued to occupy his close attention until his death. The great powers which he possessed as an organiser and master of detail ensured the advancement and complete commercial success of this great establishment, and rendered of real efficiency his unceasing efforts to promote the physical and moral welfare of his workpeople.

Mr. Spottiswoode began to communicate to the world the results of his mathematical researches in 1847, when he issued a series of five pamphlets, which he entitled "Meditationes Analyticæ." These contained thirteen essays on a variety of mathematical topics, including the curvature of surfaces, virtual velocities, infinitesimal analysis, physical astronomy, and the calculus of variations. After a pause of three years, he in 1850 sent three brief papers to the "Philosophical Magazine" on quaternions, and from that time forward his communications to the leading English and foreign mathematical societies and journals were poured out in an almost continuous stream. In a list of his publications which is before the writer, four years only of his subsequent life, viz., 1858, 1867, 1869, and 1878, appear without some record of mathematical work committed to the press. Much of this no doubt was of a slight and fugitive character, consisting merely of new proofs by elegant methods of known theorems, or notes of ideas suggested by the papers of others which the wide range of his college reading enabled him to follow without difficulty, so that but few were allowed to escape his attention. But of important and original work there was an abundance. The interesting series of communications on the contact of curves and surfaces which are contained in the "Philosophical Transactions" of 1862 and subsequent years, would alone account for the high rank he obtained as a mathematician. It would not be possible to discuss in any detail the various mathematical writings which have established the reputation of Mr. Spottiswoode without the use of complex symbolical expressions quite inconsistent with the objects of a brief obituary notice. In truth, the mastery which he had obtained over the mathematical symbols was so complete that he never shrank from the use of

expressions, however complicated, nay, the more complicated they were the more he seemed to revel in them, provided they did not sin against the ruling spirit of all his work—symmetry.

To a mind imbued with the love of mathematical symmetry the study of determinants had naturally every attraction. In 1851 Mr. Spottiswoode published, in the form of a pamphlet, an account of some elementary theorems on the subject. This having fallen out of print, permission was sought by the editor of "Crelle" to reproduce it in the pages of that journal. Mr. Spottiswoode granted the request, and undertook to revise his work. "The subject had, however, been so extensively developed in the interim, that it proved necessary not merely to revise it, but entirely to rewrite the work," which became a memoir of 116 pages. To this, the first elementary treatise on determinants, much of the rapid development of the subject is due. The effect of the study on Mr. Spottiswoode's own methods was most pronounced; there is scarcely a page of his mathematical writings that does not bristle with determinants.

Two communications made by Mr. Spottiswoode in 1860 and 1863 to the Royal Asiatic Society upon mathematical subjects, should be specially referred to. In a brief note in the Journal of that Society (vol. xvii, pp. 221—222) he discusses the claims of Bhóskaráchary, an Indian astronomer, to the discovery of the principle of the differential calculus; and in a more lengthy article in vol. xx, pp. 345—370, of the same publication, he translates into modern symbols the formulæ made use of by the Hindoos in calculating eclipses, contained in the "Súrya Siddhánta." The acquaintance which he had with this work was formed by reading it in the original tongue, for among his varied acquirements he possessed a remarkable knowledge of several European and Oriental languages.

Mr. Spottiswoode was not a traveller in the usual extensive meaning of the term, but he has left us an interesting record of a journey which he made in 1856 through Eastern Russia, entitled "A Tarantasse Journey through Eastern Russia in the Autumn of 1856;" and in 1860, in company with his brother and a sister, he accomplished an expedition through Croatia and Hungary.

In 1861 Mr. Spottiswoode was married to the eldest daughter of the late William Urquhart Arbuthnot, a distinguished member of the Indian Council.

In 1871 Mr. Spottiswoode turned his attention to experimental physical science. The resources at his command enabled him to furnish his laboratory on a scale which rendered it in some respects unique. The gain to the scientific world was not due merely to his own experiments, as, with characteristic generosity, he was always ready to advance the discoveries of others by the loan of the costly and beautiful apparatus with which he had surrounded himself, and

in the perfecting of which he had spent much care and ingenuity. His earliest researches bore on the phenomena of the polarisation of light, upon which he wrote an admirable little handbook, published in the "Nature Series." At a later period he made a number of communications to the "Proceedings of the Royal Society" on the electric discharge in rarefied gases. In 1879 he was joined in his researches on this subject by Mr. J. F. Moulton, and in conjunction with him entered upon an investigation of the sensitive state of the discharge. An important paper in the "Philosophical Transactions" of 1879 (pp. 165—229), and some shorter notes subsequently published in the Society's "Proceedings," give in detail the singular and elegant results which were arrived at.

The great beauty of the experiments involved in Mr. Spottiswoode's physical researches led to demands from his friends that they should be laid before the public in a popular form. The lectures which he delivered to crowded audiences at the Royal Institution and elsewhere were characterised by a remarkable clearness of exposition, and by a depth of poetic feeling which excited much surprise among those who knew of him only as an abstruse mathematician. Perhaps the most interesting example of his powers as a lecturer is to be found in a discourse on "Sunlight, Sea, and Sky," delivered to working men at the British Association Meeting in Brighton in 1872 ("Nature," vol. vi, pp. 333—336). The reputation he acquired in these essays excited high expectations with regard to the address which, as President of the British Association, he had to deliver in Dublin in 1878. These expectations were fully justified by the result. The stores of a mind imbued with the spirit of a philosopher, a mathematician, a physicist, and a poet, were drawn upon with no niggard hand, and matters usually regarded as beyond the ken of others than experts were explained to the unversed in language as interesting as it was simple, clear, and precise. The judgment of his fellow-workers could now be unhesitatingly approved by others.

The honours which were bestowed on Mr. Spottiswoode were many. He was LL.D. of Cambridge, Dublin, and Edinburgh, and D.C.L. of Oxford. He was elected correspondent of the Institute (Académie des Sciences) for the Geometrical Section, after a sharp contest with M. Borchardt. He was Fellow of the Royal Society of Edinburgh, the Royal Astronomical Society, the Royal Asiatic Society, the Royal Geographical Society, the Society of Antiquaries, and the Ethnological Society. He occupied the Presidential Chair of Section A of the British Association in 1865, of the London Mathematical Society in 1870–2, of the British Association in 1878, of which latter body he acted as Treasurer from 1861 to 1874. Of the Royal Institution he was Treasurer from 1865 to 1873, and Secretary from 1871 up to his death. He was also a Trustee of the British Museum.

The Fellowship of the Royal Society was conferred upon him in 1853. After having served several times upon the Council, he in 1871 became Treasurer, a position which he held up to 1878, in November of which year he succeeded to Sir J. Hooker as President. One more fully qualified to occupy this important post it would be difficult to find. To a manner in which sweetness and dignity were singularly blended, he added an unfeigned interest in the work of others, in whatever field it lay, and a rare quickness of appreciation of its merits; while his love of society and liberal hospitality had surrounded him with a wide circle of distinguished friends both English and foreign.

Death overtook Mr. Spottiswoode while he was yet in the prime of life and in the full vigour of his intellect. A serious tricycle accident some months before had lowered his strength so that he was unable to resist an attack of Roman fever, complicated by congestion of the lungs; and thus he passed away on the morning of June 27, 1883.

His remains rest in Westminster Abbey. If further words are needed to justify the claims of William Spottiswoode, President of the Royal Society, to such an honour, none better or more eloquent could be selected than those uttered over his scarcely closed grave by the Dean of Westminster, words which supply that reference to the beauty of his character, without which this brief sketch of him would be indeed imperfect. "Those to whom his memory is dear need not blush to think that he lies near those whose thoughts have enriched, whose examples have guided, or whose lives have served mankind; that he rests there not as a thinker only, not as a student only, but as a citizen of England, as a gifted worker in the fair domain of knowledge, as a busy worker in the manifold range of active life, and that he carried into each sphere the same minute and careful and constant and untiring industry, the same rare powers, the same high aim of serving truth, of serving man, and of serving God. . . . He was emphatically by nature and by choice a man of science. His own special and more cherished studies lay in those high and abstract regions which are traversed only by the few. He moved with ease, we are told, on heights where others can scarcely draw their breath. He did not devote himself to those great fields of knowledge, success in which at once appeals to the imagination of us who stand outside the circle of the true students of science. We can point to no marked and tangible result of his labours such as comes at once to the mind of the passing visitor, or may be brought home to the comprehension of even the least instructed stranger, as he stands on the grave of a Newton, or a Herschel, or a Lyell, or a Darwin. Yet he was, in the truest sense of the words, a man of science. Devout in soul and unperplexed in faith, he never, we are told, cared for one single moment to speak of science in the sense implied in the often misused

phrase as 'the handmaid of religion.' He looked on the study of science as, I might even say, part of his religion, as the pursuit of truth, truth only, truth for her own sake; a pursuit to be followed up independently, fearlessly, faithfully, to whatever results patient and enlightened, but impartial and honest, investigation should lead the inquirer. He shrank from all attempts to divert, or to confuse, or to limit the aim of the student by putting before him any other one consideration than that of the pressing forwards to what was clear, true, and demonstrable within his own department. . . . And to all who followed truth in the same spirit he turned with an instinctive and cordial sympathy. He won men's hearts, we hear, by an unassuming and unselfish gentleness. But he did more. The variety of mental powers which enabled him to hold the threads of the many branching lines of the ever expanding studies of his age, which touched with poetry his treatment of the most abstruse themes, and gave a rigorous accuracy to his management of the smallest detail of his business, was united to an attractiveness and transparency of character, and a spotless integrity and uprightness which secured men's confidence. . . . And if he lies not far from those whose genius—very different to his own—has enlarged the bounds of human thought, and embodied sometimes in immortal clothing the various chords of human feeling and emotion, it is something to feel that rarely beneath this roof has been laid one of a purer or more spotless life."

A. B. K.

Peter William Barlow, whose death occurred on May 19th, 1885, was the eldest son of the late Professor Barlow.

He was educated at private schools, and having at an early age selected civil engineering as his profession, became a pupil of the late Mr. Henry Palmer, Member of the Institute of Civil Engineers, under whom he was engaged on the Liverpool and Birmingham Canal and the then New London Docks.

The active demand for railways which followed the opening of the Liverpool and Manchester Railway, caused him to be employed in the preliminary surveys and studies of the county of Kent, with reference to a railway to Dover, and in 1836 he acted as resident engineer under the late Sir William Cubitt, on the central division of the London and Dover Railway, which formed the nucleus of the present South Eastern system. He subsequently became resident engineer of the whole line, and afterwards the engineer-in-chief, during which period he constructed the North Kent, the Tunbridge and Hastings, and many other lines in connexion with the South Eastern system. He also constructed the Londonderry and Enniskillen, and the Londonderry and Coleraine, and other railways.

As illustrating the activity and energy with which railway enterprise was carried on at the time, it may be mentioned that the Tunbridge Wells Branch, which forms the first portion of the Tunbridge and Hastings line, was carried into execution by consent of the landowners and occupiers before the Act of Parliament which authorised its construction was obtained.

In 1858 Mr. Barlow investigated in great detail the construction of bridges of large span, especially with the view of stiffening the roadways of suspension bridges, and in pursuance of those studies he went to Niagara, in order to examine personally the great railway and road bridge erected there by Roebling.

Mr. Barlow's latest public engineering works were the Lambeth Bridge, which has since been purchased by the Metropolitan Board of Works, and the Tower Subway, the latter work having attracted much attention from the originality of its conception and the cheapness and rapidity of its construction. This subway is formed of cylindrical rings of cast iron, and by forcing forward a cylindrical shield, it was constructed from the north to the south side of the river (900 feet) in fourteen weeks. Although of small dimensions, and only adapted for foot passengers, it is extensively used by the working classes, who were formerly entirely dependent on the ferry.

After the completion of these public works, he erected an iron bridge on the River Lea, and had surveys made for a tunnel under the harbour of Rio de Janeiro for the Brazilian Government.

In 1881 he suffered under an attack of cataract, which entirely deprived him of sight for several months. A successful operation restored the use of one of his eyes sufficiently to enable him to read. He was, however, no longer able to pursue his professional career with the zeal and energy which characterised the earlier part of his life.

Mr. Barlow was the author of several scientific papers, among which the following may be mentioned:—

"An Investigation of the Laws which govern the Motion of Steam Vessels," "Phil. Trans.," 1834.

"On the Strains to which Lock Gates are subjected," "Civ. Eng. Inst. Trans.," 1836.

"Investigation of the Power consumed in overcoming the Inertia of Railway Trains," "Proc. Roy. Soc.," 1846.

"On some Peculiar Features of the Water-bearing Strata of the London Basin," "Civ. Eng. Inst. Proc.," 1854.

"On the Mechanical Effect of combining Girders and Suspension Chains," "Brit. Assoc. Rep.," 1857.

"Observations on the Niagara Bridge," "Frank. Inst. Jour.," 1861.

He became a Fellow of the Royal Society in 1845. He joined the Institution of Civil Engineers in 1827, and at the time of his death was the oldest Member of that Institution. W. H. B.

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